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R. A. F. PENROSE, JR., MEMORIAL LECTURE

ECONOMIC ANALYSIS AND POLITICAL SYNTHESIS

EDWIN G. NOURSE

(*Read April 20, 1950*)

THE prime task of the economist—first in time and basic in character—is to analyze the economic process. His purpose in so doing is to get a functionally significant classification of the discrete actions of its myriad participants as a basis for tenable, even though provisional, generalizations as to the nature of major and minor economic forces and the manner of their operation. That done, he attempts to formulate these generalizations into principles or scientific "laws" to be used and tested in the further study of economic phenomena.

ECONOMICS AS APPLIED SOCIAL SCIENCE

There is relatively little place for the "pure" scientist in economics. We are not in quest of ageless and timeless laws, which will have universal applicability to a changeless subject matter like the solar or the cosmic system or like chemical elements and physical forces. We must deal with the aspiring and the rebellious, the competitive and the cooperative, the sometimes patterned but often random phenomena of human minds and souls. Moreover, we meet these phenomena not in fixed or fully repetitive situations but in a constant flux of evolving and often contradictory developments. Our profession, therefore, partakes largely of the nature of applied science or engineering. We seek to derive and apply generalizations valid for particular subject matter, limited as to both time and place. These understandings, such as they are, we use, like the engineer, in the designing and adjusting of social machines—that is, institutions—through which alone these forces can work, be directed, and to some extent be controlled.

But the economist's task does not stop there. He undertakes to employ his progressively improving generalizations or principles in the devising of economic policies, public or private. These policies furnish the logical framework under which individuals and organized groups develop concrete programs and specific plans of action. Such policies, programs, and plans seek most effective utilization of the fundamental economic forces to the ends of human life through mechanisms devised for their application.

Only casual references will be made in the present discussion to economic forces as such or the means of identifying and measuring them. The key words of my discourse will be "institution" and "policy." Furthermore, I shall be concerned mainly with a single illustrative case under each of these rubrics, namely the Council of Economic Advisers as an institution and sustained high-level production and the maximization of human freedom as a double-barrelled policy.

Economics is a social science, and both its researches and its practice need to be carried on in partnership with the work of the political scientist, the historian, the psychologist, the statistician, the sociologist, and even the anthropologist. Of these, the working relationships between economics and political science or public administration are the closest. In fact it is often said that economics today has returned or is more and more returning to its first form and orientation—as political economy. This statement is only partially true.

There was a time in the early days of the Industrial Revolution when the doctrine of *laissez-faire* demanded a sweeping withdrawal of government from the economic field. This was in protest against the cramping pressures of economic status in the area of labor relations, property ownership, and trading opportunity and against a meddlesome mercantilism in the area of public finance and commercial relations. But the growth of industrialism itself created a demand for manufacturing, mercantile, financial, labor, and agricultural organizations of a size and complexity that required government to become active in the provision of new legal structures more adequate to the performance of their burgeoning functions. It also demanded new regulatory agencies, judicial and administrative, for the protection of participants and public against serious misuse of these great new private powers. Furthermore, the demands of new tech-

nology have, from time to time, called for capitalistic development of productive resources on a scale which has transcended either the financial power or the daring of any private organization. Grand Coulee and Oak Ridge furnish recent outstanding examples. We have thus been confronted with the alternative or, in the true sense, dilemma of allowing progress to languish or of utilizing government to enter as a facilitating agency into areas traditionally reserved to private business.

We Americans have not paused very long when confronted by such dilemmas. We are a practical rather than an ideological folk and have nonchalantly practiced a "mixed system" while formally professing abhorrence of socialism and of all things socialistic.

We have, generally speaking, given the individual and the privately organized group the first chance to perform a given economic service and to reap its gains—within general codes of proper conduct. But when it has become clear that private resources are insufficient or private venturesomeness too feeble or the terms of private trade incompatible with social need, we have quite freely invoked community organization, state activity, or federal aid or even outright operation. Sometimes—as in the carrying of the mail—the people have prejudged the case and launched a public agency without giving much opportunity for private business to show what it could do. Sometimes, on the other hand, the public has put up with poor service because of a feeling that the field should, as a matter of principle, be left to private exploitation. In a surprisingly large number of economic functions we have organized along parallel lines of private and of public activity. We have accelerated or checked the expansion of postal, highway, education, health, power, forest, and amusement services as local and temporary circumstances seemed to require. It is nonsense to say that we have had any consistent or rational policy or economic or social theory in these matters. We have been opportunists trying to get something done, and to *preserve freedom of action of the citizen as well as the business enterpriser* in the process of getting it done.[1]

Out of this rather inchoate economic and political situation emerged the Employment Act of 1946. It reaffirmed and made more explicit and inclusive than ever before a national policy of adhering to a system of predominantly private enterprise. But it linked with this a policy statement more explicit and sweeping than any ever before included in federal legislation that the national government shall follow such lines of positive action as in the judgment of the legislature, under the leadership of the Chief Executive, shall be needed to maintain or restore the health of the economy.

The distinctive thing about the Employment Act is its recognition of the need for comprehensive and internally consistent economic policy as among government agencies and activities, as among private agencies and activities, and between governmental and private parts of the economy. It does not conceive our modern economic process as merely a private enterprise system seeking to attain efficiency and preserve individual freedom under voluntary representative government. It recognizes the subtle fact of an ubiquitous interpenetration of our economic and political systems. It was probably recognition, at most subconscious, of this advanced stage of evolution in our economic affairs that caused certain imaginative citizens and legislators to push at that particular time for such a development within our frame of government as the Employment Act of 1946.

The elaboration of our corporation and union institutions in the industrial field, the peculiar requirements of our family farm system of agriculture, and the adventitious forces of war and its financing with our new central bank machinery for monetizing a debt however large, conspired to create an irresistible demand for a much better administrative machinery for the economy.

SCIENTIFIC METHOD IN POLICY MAKING

In some quarters, there was a broad understanding of these courses of our economic evolution and a groping for more adequate means of responding to or dealing with them. In others, there was the more limited and definite feeling of need for a specific remedy to meet the threat of a postwar business depression. It was this latter influence which produced the Murray "full employment" bill, later superseded by the Employment Act of 1946. The Murray bill proposed a fiscal specific for the undulant fever of boom and depression, which it was widely feared would display a typical and critical postwar manifestation. This first formulation of the stabilization proposal met sharp and widespread opposition. After a multitude of counsellors had contributed their joint wisdom, the final act was framed in much more eclectic terms.

So revised, the Employment Act of 1946 for-

[1] Nourse, Edwin G., Public Administration and Economic Stabilization, paper delivered at the annual meeting of the American Society for Public Administration at Washington, D. C., March 14, 1947.

mulates a general goal and pattern of satisfactory achievement in the economic field and provides two specific devices to aid in attaining it—a Council of Economic Advisers in the Executive Office of the President and a Joint Congressional Committee on the Economic Report of the President. The goal has often been interpreted as curing or at least greatly moderating the malady which threatened to become chronic in modern industrial society, namely, the business cycle. Its objective is in fact, however, somewhat more than this. The act demands of an intelligently administered economy not merely a steady level of operation but likewise a high level of sustained use of the nation's resources, human and material—in the phrase of the act "maximum employment and maximum production."

As to the attainment of the goal thus defined, there is in this measure an at least latent assumption that we have now gained scientific knowledge and practical competence which should enable us to perfect our institutional machinery, clarify our policies, and improve our practices in handling our national affairs so that the desirable results envisaged in the preamble to the act could be reasonably approximated. In a word, the act invites economic science—or, if that be an over-ambitious term, scientific methods as applied to the subject matter of economics—into an ancillary position of recognized importance within the frame of voluntary government. Nor is this service to be rendered merely as to the government's own fiscal, monetary, or regulatory operations. It is conceived also as furnishing intellectual leadership and coordination in the reciprocal area of private business policy and practices.

By inviting economics into a larger place in the public service, the Employment Act opens the door to professional economists through the Council of Economic Advisers to apply all their techniques of analysis to the affairs of the economy. Economic (and statistical) techniques are to be used by the Council also to synthesize an ideal policy for the economy as an integrated whole. This synthesis is to be presented for consideration by the President and his official family.[2]

[2] Through the Joint Economic Committee of the Congress, economists are likewise to participate in an independent analysis of the problems and the President's proposals for dealing with them and to synthesize the Committee's own program as a frame of reference for coordination of the legislative process in so far as it touches economic matters. Space does not permit examination of this institutional development here. Reference to one or two phases of its work will be made later in my paper.

There are thus two separate and successive levels or patterns of synthesis. The first is to be seen in the confidential report of the Council to the President, appropriately labeled "materials for the use of the President in the preparation of his Economic Report to the Congress." The second is to be seen in the state paper which the President, with the collaboration of his Cabinet officers and agency heads and with the assistance of the White House staff, prepares and submits to the Congress. The former of these documents embodies the economic scholarship of the Council members as surrogates for their staff and for the profession as a whole. The latter embodies the politico-economic statesmanship of the chief executive of the federal government and the titular head of the incumbent party. I wish to describe briefly what seem to me the ideal possibilities of both documents, and also the possibility or danger that either or both of these works of art may fall slightly or seriously below levels presumably attainable in the present "state of the arts"—of business and of government.

The policy statement that the Council prepares for the President's consideration must deal with many aspects of the economy. It must make proposals for dealing with all of them simultaneously and in interaction in ways that are mutually consistent and that would, together, promote the well-being, that is the sustained productivity, of the economy to the highest degree possible. The attainment of such an economic synthesis calls for exceptional intellectual capacities and personal qualities. They may be classified under three heads.

First, it presupposes technical competence of the first order on the part of each Council member (and likewise skill in selecting and organizing a specialized but coordinated staff to supplement such competences as the Council itself possesses). Besides sheer technical ability, there is required the greatest possible frankness and objectivity. Each member of the Council (and top staff) must be able and willing to lay all the premises and the processes of his analytic and synthetic thinking unreservedly on the Council's work table for the searching scrutiny of his fellows.

Second, synthesis of a sound and workable national policy requires the greatest realism in fitting the mechanics of economic theory to the human pe-

culiarities of the persons through whom the actual economic process is conducted. The economist is dealing, in the future tense, with the mass behavior of 150 million technically free but variously conditioned human beings and the individual behavior of a much smaller, though still considerable, number of executives who wield greater or less degrees of administrative power or influence.[3] His cannot be an exposition which traces cause and result sequences with the mathematical precision of the natural scientist, who deals with inert and tractable materials and orderly and measurable forces. He must show in his synthesis a realistic grasp of whatever the psychologist has taught about normal and abnormal psychology plus such insights about incentive, motivation, and revolt as he has been able to gain from empirical labors in his chosen field. Here is a fine line, to attain psychological realism and yet not be swayed by subjective judgments.

Third, the making of its economic synthesis demands of each Council member a broad and deep social philosophy. Only so can he weigh and scale the demands of the several parties to the economic process and display a calm judicial quality as the economic attorneys for one or another special interest group argue the claims of their respective clients for favored treatment in the matter of income, for special relief from effort or responsibility, or for exceptional security. These issues will frequently present economic dilemmas. So confronted, the Council must synthesize a policy which "on balance" holds greatest promise of practical workability. Furthermore, this must be presented in a way which will promote understanding and acceptance by the several contending parties.

Having sketched what seem to me the limits of proper economic synthesis and the qualities required of the Council members in reaching or approximating that ideal, I now turn to two comments on how or why that level of attainment may not actually be reached. They concern first the availability of material from which to choose, and second the lack of understanding of needed qualities on the part of the President in appointing, or the Senate in confirming, candidates for the Council.

One of the greatest shortcomings in my profession is a superindividualism which causes many of its members to develop each his own economic system or philosophy, expressed in a trick vocabulary. There has also been a deplorable willingness of some members to become the attorneys for certain vested interests or interests aspiring to become vested. It may likewise lead to more or less active identification with a political party. Third, and related to the points just mentioned, is an unwillingness on the part of many persons of scholarly pretensions to accept the anonymous and self-abnegating role which is called for by the peculiarly confidential relationship of a professional staff post in the Executive Office.

If we are to have economic synthesis developed calmly but vigorously on its side of the line, in such a way as to have greatest practical helpfulness and, at the same time, greatest disciplinary value to political synthesis on the other side, the economist must be spiritually capable of bringing the choicest pearls of scientific work to cast before the politically motivated and politically conditioned policy makers of the Executive Branch. He must be prepared to see these carefully fabricated materials rejected or distorted, and still carry on the same process of preparation and submission again tomorrow, unperturbed and unabashed. He must all the while be aware that his professional brethren and the public will hold him accountable for the final compromised product while he, by virtue of his relationship to the Executive Office, is estopped from saying anything in explanation or vindication of his own workmanship. Many an able economist refuses such an affront to his "academic freedom" and professional dignity.

Much also devolves upon the President if a Council is to be provided which will cap the most competent economic analysis with the most adequate economic synthesis. The Employment Act stipulates that the Council "shall be composed of three members . . . each of whom shall be a person who, as a result of his training, experience, and attainments, is exceptionally qualified to analyze and interpret economic developments, to appraise programs and activities of the government . . ., and to formulate and recommend national

[3] Insight into the psychological processes of business executives, labor officials, farm leaders, and organized consumers was promoted through a number of consultative committees set up by the Council. As these representatives of the various groups sat around our Council table, it was possible first to draw out their criticisms of government policies or the action of rival groups and to have them elucidate their own objectives and the means they proposed for attaining them. It was possible also to get their reaction to difficulties as revealed in Council analyses and the probable workability of policies under consideration by the Council or by particular government agencies. Finally, it was possible to get from them some expert counsel as to the reasoning, attitudes, and probable behavior of the rank and file in their respective groups.

economic policy." This implies that the President shall have a real grasp, direct or vicarious, of the qualities needed for scientific performance and a sensitive desire that service shall be kept on that plane. It requires also that he shall subsequently in every way emphasize and preserve the non-political character of the agency. Since members will only in exceptional cases have been entirely free of active affiliation with political or other policy-promoting organizations, the President should insist that, after appointment, they regard Council membership as enjoining withdrawal from these connections to an extent comparable to that expected, in their somewhat analogous area, of Justices of the Supreme Court. For the President to permit or encourage intervention of Council members in any manner in the legislative process or in the public promotion of measures or policies is to make them politically expendable and to destroy the unique usefulness of the Council as an institution designed for scientific service to the Executive Branch.

The title of my paper, in the interest of brevity, was compacted to the form "Economic Analysis and Political Synthesis." The scheme of treatment in fact involves three concepts: economic analysis, economic synthesis, and political synthesis. Among these three I make a two-way comparison. The first comparison is between economic analysis and the synthesis of a comprehensive, realistic, and objective many-faceted policy based strictly on criteria of economics as a social science. The second comparison is between such a policy and an active political policy designed to combine strictly economic considerations with others which cannot be ignored. This second level of synthesis, which enters into the making of a national economic policy under the objectives set and with the devices provided in the Employment Act, inevitably is the personal responsibility of the President.

Under our system, the President is not merely the chief executive of a democracy. He is also the titular head of a political party under a two-party system, and this we hold to be the best system yet devised for free government. It is obvious that the President cannot, as a practical matter, adopt and effectuate a policy drawn up exclusively in accord with economic criteria even in the realistic view I previously referred to. I used often to say to staff members: "If you were translated from your relatively comfortable office chair to the hot seat occupied by the President, you would not find it wise or safe as political head of the state to make your statesmanship conform precisely to what, strictly as an economist, you would be profoundly convinced was the desirable course."

I recognize that the line between the realistic economist and the scholarly politician is so vague as not to be drawn with even an approach to precision. It seems to me, however, that it is quite possible to perceive the qualitative difference between (*a*) political synthesis of a statesman's policy based on a conscientious study of the economic synthesis of competent technicians on his staff and (*b*) the political disregard for or debauchment of economic data, analysis, and objective evaluation. Synthesis is an orderly process which observes rules of evidence, scientific canons, and strict logic to test cause-and-result relations. It is very different from the cunning of a politician seeking to capture votes by specious or venal appeals in order to remain in power, synthesizing a plurality by the additive process of placating special interest groups. In the vernacular, this might be called "drawing to fill an inside straight."

There is one quite simple and obviously valid criterion of a proper political synthesis: What combination of segmental policies will integrate into an overall policy which will be not only economically self-consistent but will, "on balance," be best calculated to promote the welfare of the whole people—while preserving their basic freedoms? In the search for such a national political synthesis the Council's position in the Executive Office must be vitalized not merely as toward the President; it must be active and intimate also as to Cabinet officers and other agency heads. The Council must challenge the policies of those political lieutenants of the Chief Executive if they seem designed or to tend toward promoting any special interest rather than maximum production or continuing well-being for the whole people. The President must be willing to preside over, and the Cabinet officers willing to participate in, a genuine seminar session for the final resolving of conflicting policies.[4]

[4] In December 1948 President Truman, at the suggestion of two Cabinet officers and one other agency head, asked me to set up a Coordinating Committee on anti-inflation policy. I thought it better to interpret this as a committee on economic stabilization policy as a more generalized purpose for what might become a permanent or recurrent activity within the Executive Office. The committee did, in fact, have another series of meetings in connection with the Midyear Economic Report of the President six months later.

I regarded this move by the President as the high-water mark of recognition of the Council and of its employment

BASIC DIFFICULTIES IN GETTING A SOUND POLITICAL SYNTHESIS

In commenting on criteria of an ideal political synthesis, I have clearly implied some of the difficulties to be encountered in reaching that high plane of statesmanship. These difficulties inhere not only in the personality of the President and of the aides whom he selects but also in certain features of our government structure and general policy. Some of the latter are deeply embedded in tradition. Examining them would require time far beyond what is available for this paper. Some of them, on the other hand, are indigenous to the current situation.

This is particularly manifest in the present drift toward central governmental planning in the implemented blueprint sense. Present political policy-making seems to be infused with a great zeal (*a*) to get quick results and (*b*) to have these results conform to a certain philosophy of "what is good for the people" rather than a desire to facilitate their working out that kind of life which they themselves will, in the long run, find good. This is much too large an issue to open up at the close of a brief paper.[5] It is, however, interesting to conjecture whether this trend will persist and grow stronger in the future. It would seem to me both to negative the town meeting concept of democracy on which the United States was founded and to negative also the teachings of the scientific method which I hoped the Employment Act would make much more effective in our executive and legislative processes.

The potential usefulness of the Employment Act, with its professional Council of Economic Advisers, has not yet been given a real test. Its lack of success has, in addition to the Council's own internal shortcomings, been due to the fact that even such materials as it has prepared were not studied and debated as a preliminary to making those compromises among rival desiderata that are a part of the art of free government. It seems to me self-evident that the Council will be accepted within the government and by the public only in the light and at the level indicated by the President. Discussing the present posture of the matter in a recent meeting of the American Economic Association, I made this comment: "The real difficulty is not that the role assigned under the law is too limited but that in this Administration the Council has undergone such progressive attrition or debasement that it bids fair soon to be negligible."

One may speculate on how this promising experiment would have fared under Franklin Roosevelt, Herbert Hoover, Calvin Coolidge, or Warren Harding. He may speculate also on what success it would have under Dewey, Eisenhower, Taft, or Stassen. One of these gentlemen, several years ago, made a pilgrimage to one of our leading universities. He invited a group of its top economists to meet with him that he might get the best of their wisdom on certain pending economic questions. He said: "Just give me the economic facts and analysis. If I have to act on these matters in the political milieu, I have, and expect to rely upon, my own techniques for making political evaluations and executive decisions." As the session proceeded, he had to become schoolmaster to the schoolmasters to keep them from spilling over into political—perhaps they called them social—value judgments far beyond the cause-and-result relationships on which he sought their technicians' aid.

It would, I think, have great value for us economists to deal with a political executive who saw this distinction clearly and enforced it as a frame of reference for the work of his economic advisers. I am not presuming to make a nomination for President or assuming that this is the only qualification. But I do suggest that not until we have a President with that kind of mind and not until he finds economists who can make professionally wise economic synthesis and refrain rigorously from political synthesis will the Employment Act and its Council achieve the high purposes which are latent in it.

In closing I repeat an observation I have made on a previous occasion: "It will take time for successive Presidents to learn how to use a nonpolitical advisory staff agency effectively. It will take time for successive Council members to learn how to bring the most competent and realistic analysis of economic problems simply and effectively to the President's aid."

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as a catalyzer of policy thinking in the Executive Branch of the government. As I presided over the meetings of that group, it was very interesting to observe the way in which executive heads of other agencies would argue against the policies proposed by a brother secretary or chairman when he overreached himself to serve his constituents through ex parte proposals that threatened the general welfare.

[5] I examined some phases of it in an article, Collective bargaining and the common interest, *Amer. Econ. Rev.* **33**: 1–20, 1943.

A POET AT WORK: CHAUCER REVISING HIS VERSES

KEMP MALONE

Donovan Professor of English Literature, Johns Hopkins University

(*Read April 21, 1950*)

THE Prologue of Chaucer's *Legend of Good Women* has come down to us in two versions, known to scholars as F and Gg. For many years philologists have debated at length about these versions, the question at issue being that of priority: which version is the earlier? For my part I follow ten Brink and many others in giving priority to version F. Without arguing the matter, and proceeding on the hypothesis that Gg represents a revision of F (a hypothesis based on evidence which I look upon as convincing but which, for want of time, I cannot present in this paper), I will compare in detail a passage of thirteen lines in version F with the corresponding passage in version Gg, in order to find out whether my working hypothesis actually works. Before accepting the hypothesis as proved one would have to do much more, of course: one would have to apply it not merely to a single passage but to the whole of the Prologue; and one would have to apply the contrary hypothesis in the same way, the choice between them being then made according to the usefulness of each in explaining the phenomena under investigation. Here I can do no more than present a sample of the complete study. This sample will serve to illustrate the procedures which I have used in my examination of Chaucer's poem.

The sample which I have chosen for presentation at this meeting is a passage about old books, lines 27–39 of the Prologue. In version F it reads as follows:

27 Wel ought us thanne honouren and beleve
These bokes, there we han noon other preve.
And as for me, though that I konne but lyte,
30 On bokes for to rede I me delyte,
And to hem yive I feyth and ful credence,
And in myn herte have hem in reverence
33 So hertely, that ther is game noon
That fro my bokes maketh me to goon,
But yt be seldom on the holyday,
36 Save, certeynly, whan that the month of May
Is comen, and that I here the foules synge,
And that the floures gynnen for to sprynge,
39 Farwel my bok, and my devocioun!

The corresponding passage in version Gg reads thus:

27 Wel ought us thanne on olde bokes leve,
There as there is non other assay by preve.
And as for me, though that my wit be lyte,
30 On bokes for to rede I me delyte,
And in myn herte have hem in reverence,
And to hem yeve swich lust and swich credence
33 That there is wel unethe game noon
That fro my bokes make me to goon,
But it be other upon the halyday
36 Or ellis in the joly tyme of May:
Whan that I here the smale foules synge
And that the floures gynne for to sprynge
39 Farwel my stodye, as lastynge that sesoun!

Chaucer's revision of this passage did not involve any major operations. The lines stayed where they were (except for 31 and 32, which exchanged places), the thought remained the same, and the number of lines in the passage was not altered. Elsewhere in the Prologue we find Chaucer making changes much more radical. But it will be worth our while to examine somewhat narrowly the changes which we find here. By comparing the two versions of this passage we can peep over the poet's shoulder, as it were, while he is at work, and learn something about one aspect, at least, of his poetic art: his technic of revision. Let us then set to work, without further preliminaries.

The most striking difference between the two versions of our passage has to do with the linear structure. In version F we find three run-on lines: 27, 32, and 36. In version Gg, all the lines are end-stopped. In other passages of the Prologue the same difference appears, and it would seem that Chaucer looked upon a run-on line as metrically inferior, even though he obviously did not rule it out altogether. We might as well begin our detailed examination with line 27, the first line of the passage and the first example of the change from run-on to end-stopped linear structure. Line 27 as it stands in version F does not

make a complete unit of thought; the thought is completed by the first two words of line 28. Chaucer's problem here was to make the thought complete within the limits of the line. He got most of the space needful for this by canceling the verb *honouren,* which was something of a metrical filler anyway, and contenting himself with one infinitive. The *and* after *honouren* had the function of linking the two infinitives. Now that one of the infinitives was canceled, the link served no further purpose and had to be canceled too. These cancellations left Chaucer with two metrical feet to fill in line 27. He might have shifted the *these bokes* of the next line into the empty space which he had made, but the line which would thus have come into being could have been given a satisfactory scansion with difficulty if at all. Chaucer therefore gave himself a little more leeway by canceling the prefix *be-* of his second infinitive, thus changing the verb-form into a simple *leve.* Into the space of five syllables thereby made available in line 27, Chaucer put the phrase *on olde bokes,* filling the line neatly enough from a metrical point of view and completing the thought beautifully within the limits of the line. The phrase actually used, *on olde bokes,* was obviously inspired by the *these bokes* of the earlier version, taking shape as it did to fit the metrical and syntactical conditions of Chaucer's problem.

In the foregoing I have made no effort to reconstruct the course of events in a sequence true to psychological reality. I do not know what this sequence was and I see no way of finding out. Chaucer may perfectly well have started by deciding to shift *these bokes* to line 27, and only then may he have looked at that line to see what he could dispense with there. But I am inclined to think that he began by brooding over his problem, and that the solution which he ended with came to him all at once, in a flash. Certainly he need not have gone through any such conscious procedure as that which I have outlined. But whether he analyzed and then synthesized consciously or subconsciously, his mind worked on the problem before he solved it, and the elements which his mind had to consider before the problem could be solved are the elements which I have pointed out. Whether these elements were considered one at a time or together we shall never know, but we do know that they were all considered.

Chaucer's problem in line 28 differed greatly from the problem he solved in line 27. When he shifted *these bokes* or the equivalent phrase to line 27, he thereby made a blank space in line 28, a space which had to be filled to satisfy the metrical requirements of the line. His problem, then, was one of expansion or augmentation. What was left of line 28 had to be given a new wording which would fill up the line. The poet had to say the same thing in more words. A little prolixity was needed. Chaucer expanded *noon other preve* "no other experience" to *non other assay by preve* "no other test by experience." This statement happens to be more precise as well as longer than the corresponding statement in version F, but the great reason for making the change in wording was to fill up the line, not to gain greater precision. In other words, the problem here was metrical, not semantic. The same applies to the expansion of the first part of the line: from *there we han* "where we have" to *there as there is* "there where there is." In this case the metrical requirements made it needful to cancel the pronoun and change the verb, but there is no real change in the thought.

We come now to line 29. Here the change amounts to little. There is no difference at all in the thought, but the wording of the revised version is such that the personal pronoun *I* vanishes from the scene. Chaucer also gets rid of this pronoun in line 31. Version F has three *I*'s in as many successive lines (29, 30, and 31), of which only one is kept in the later version, the one in line 30. This change is to be reckoned a stylistic improvement, an avoidance of repetition.

Line 30 is left unchanged. Lines 31 and 32 are made to change places and the first two words of line 33 are canceled. These words, *so hertely,* belong grammatically to line 32 and their occurrence at the beginning of line 33 keeps line 32 from being end-stopped. Their cancellation, then, turns line 32 (or line 31, as it becomes in version Gg) from a run-on to an end-stopped line. But the idea behind the canceled words needs expression, and Chaucer found means to get this idea in without giving up the systematic end-stopping characteristic of the revised version of the passage. To express the idea he used *swich* "such" (that is, "so much"). He found he could not insert *swich* into line 32, because doing so would destroy the metrical pattern of the line. He therefore put line 32 immediately after line 30, and brought the old line 31 down to the position of line 32. The pattern of this line (the new 32) included a word-pair connected by the conjunction *and,* and Chaucer kept the pairing, though he changed one member of the pair. *Credence,* the second member of the

pair, was modified by the adjective *ful,* and it was easy to substitute *swich* for this *ful;* there is even a connection in meaning, though of course not a complete equivalence. The first member of the pair, *feyth,* had no modifier, but before it came an *I* which could be dispensed with, and Chaucer put another *swich* where the *I* had stood. He had to have two *swich*'s for the sake of symmetry, one modifier for each member of the word-pair. But "swich feyth and swich credence" did not fully satisfy him, in spite of its symmetrical structure. The two members of the word-pair mean very much the same thing, and Chaucer saw no point in the semantic repetition. The second member of the pair, *credence,* was the rime word and could not be changed, but *feyth* was easier to handle. Chaucer replaced it with *lust* "interest," a very satisfactory solution indeed.

With *so hertely* canceled, the rest of line 33 needed expansion, the problem here being like that in line 28. Chaucer turned the trick readily enough, by inserting the adverbs *wel* and *unethe* before *game.* This changed the meaning a little, but so little that it hardly mattered. Line 34 was left as it had stood, except for one subtle change: the indicative *maketh* was replaced by the subjunctive *make.* This change goes with the *wel unethe* of line 33; in both cases the statement, unqualified in version F, has become slightly less positive.

In both versions the concluding lines of the passage specify the exceptions to the rule set down in lines 33–34. These exceptions are two in number: the holiday and the month of May. In version F the two exceptions are not on the same footing: Chaucer said he "seldom" left his books on holidays but he made no such qualification about his behavior in May. In version Gg, however, Chaucer treated the two exceptions in the same way: he marked their equality by using the correlative pair *other . . . or ellis* "either . . . or else." The first member of the correlation, *other,* replaces the *seldom* of line 35; the second member, *or ellis,* replaces the *save, certeinly* of line 36. The correlation carried with it a parallelism in the structure of the two lines, a parallelism not to be found in the earlier version, where line 35 ends with a prepositional phrase whereas line 36 ends with a clause which runs on into the next line. Chaucer took the consequences of the correlation which he had set up and made the two lines parallel in structure throughout: "upon the halyday" (line 35) answers in formal pattern to "in the joly tyme of May" (line 36), though the correspondence is not absolutely rigid, since the second prepositional phrase has a somewhat more complicated structure than has the first. Both lines, too, are end-stopped in the later version, whereas in version F only line 35 has end-stoppage. The change from *month* to *tyme* in line 36 has the effect of removing the alliteration, and this may have been Chaucer's reason for making the change; in the first line of the poem the change he makes has the same effect. The insertion of *joly* into line 36 solves happily enough the metrical problem here.

The last three lines of the passage are integrated with line 36 in the earlier version, but this integration could not be kept if lines 35 and 36 were to be correlated and made parallel. Chaucer therefore took the other tack and made the last three lines an independent grammatical unit, serving to tell us more about what happened to him in the jolly time of May but giving us this information without grammatical linkage to line 36. The significant changes here are the cancellation of *is comen* and the replacement of *and* by *whan.* These two changes, both in line 37, remove the two links which in version F bind lines 36 and 37 together. Line 37, shortened by the cancellations, was made metrically complete by inserting the adjective *smale* before *foules.* It might be added that the new line 37 scans better than the old, but the *comen* of version F, which makes the metrical difficulty, is probably nothing more than a scribal error for the metrically correct *come* (past participle). Line 38 was left unchanged. The changes made in line 39 were not needed, so far as I can see. I wish Chaucer had let the line stand as it was. But there is no accounting for tastes.

If now we compare the two versions of the passage as a whole, we see plainly enough that the Gg version is structurally tighter, more polished, showing a greater degree of balance and symmetry in its parts. In particular, lines 35 and 36 have an awkwardness in version F which is not to be found in version Gg, and the change from run-on to end-stopped lines adds to the finished effect. From a strictly technical point of view version Gg is clearly superior, and it would be hard to imagine the poet changing his verses in the contrary direction: making them less polished, rougher, adding a touch of awkwardness and the like. In other words, the evidence to be found in this passage bears out the hypothesis that F is the earlier, Gg the later version of the Prologue.

It remains to consider the effect of Chaucer's changes on the poetical values which the passage

has to offer. I have already expressed my opinion that the poet ought to have left line 39 unchanged. The other changes, though they make the verses smoother, do not improve the passage poetically, so far as I can tell. Indeed, they take away something of the freshness and spontaneity so marked in the earlier version. I realize that this judgment of mine is subjective and perhaps I am wrong in bringing up such matters at all on this occasion, devoted to scientific rather than to impressionistic literary study. I yield to temptation here because I am deeply interested in classifying Chaucer as a reviser. Some authors work hard and long at revision without gaining anything thereby; Balzac is a well-known case. Others in the course of revision may have inspirations which lift a passage from verse to poetry. In my opinion, Chaucer falls into neither of these groups. He gains, as we have seen, in polish, but I think he loses more than he gains. Chaucer's best effects go with the colloquial, informal style of which he is master. Any attempt to polish such verse as his may be perilous indeed. A little polish is a good thing, no doubt, even in the colloquial style. But when you set out to polish, if you don't watch out you will overdo the thing, with disastrous consequences to your verses. Chaucer's genius, if I mistake not, found better focus in the heat of original composition than in the cold of critical revision.

PRE-COPERNICAN ASTRONOMICAL ACTIVITY

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(*Read April 21, 1950*)

FORMER MISAPPREHENSIONS

Not many years ago the notion was widespread and even generally held that Peurbach (1425–1461) and Regiomontanus (1436–1476) marked the first gleams of reviving astronomy in Christian western Europe and prepared the way out of the desert of medieval darkness and ignorance for the epoch-making work of Copernicus.* Yet a century of labor and investigation by historians of medieval astronomy, from Delambre in 1819 [1] to Duhem [2] in 1915–1917,[2] had already undermined this position, which further research in later years has left without foundation and completely demolished.

It is not, however, the primary purpose of this paper to review ground already covered, to go back, for example, of Peurbach and Regiomontanus at Vienna to John of Gmunden in the early fifteenth century,[3] or, in the closing years of the thirteenth century, to Guillaume de Saint-Cloud at Paris, already discussed in 1869 by Littré,[4] or to the Alfonsine Tables of about 1270—which, however, were not mentioned by Guillaume de Saint-Cloud, writing in 1292 and 1296, and which seem to have spread outside the Spanish peninsula only in the fourteenth century.

Incidentally let me note that as late as 1893 Norbert Herz, in a paper in the *Sitzungsberichte* of the Vienna Academy, could assert that the Alfonsine Tables were the last to be drawn up before the invention of printing.[5] In reality new Tables appeared frequently during the period between Alfonso the Wise and the invention of printing, as we shall soon indicate.

RECENT MANUSCRIPT MATERIAL

The present intention is merely to illustrate how great was the interest and how widespread the activity in the field of astronomy in the fourteenth and fifteenth centuries by some further specific examples, encountered in manuscripts, for the most part during the past summer at the Bibliothèque Nationale of Paris.[6] My search was by no means exclusively or even primarily for such instances of astronomical activity. Rather were they met with incidentally and were, so to speak, chance discoveries and by-products. For example, out of 115 manuscripts examined last summer at the Bibliothèque Nationale, only eighteen will be cited here in connection with our present subject. Of these eighteen, however, a majority were composite manuscripts—astronomical collections and libraries in miniature rather than single works. I also read last summer in the libraries of Berne and Basel, and shall have occasion to refer to three manuscripts in those collections. A few manuscripts in other libraries will be mentioned incidentally.

It should be further understood that the examples of astronomical activity which we are about to examine do not duplicate any of those already noted in my collection of Dates in Intellectual History of the Fourteenth Century,[7] whose first year

* Joh. A. Repsold, *Zur Ceschichte der astronomischen Messwerkzeuge von Purbach bis Reichenbach, 1450 bis 1830,* Leipzig, 1908, passed immediately from instruments of the school of Alexandria and instruments of the Arabs to Peurbach and Regiomontanus.

[1] Delambre, J. B. J., *Histoire de l'astronomie au moyen âge,* Paris, 1819.

[2] Duhem, Pierre, *Le système du monde,* especially Tomes 3–5, Paris, Librairie scientific A. Hermann et Fils, 1915–1917.

[3] The most recent treatment of John of Gmunden is in English by John Mundy: *Isis* **34**: 196–205, 1943.

[4] *Histoire littéraire de la France* **25**: 63–74. See further Duhem, *op. cit.* **4**: 10–19.

[5] Herz, Norbert, Ueber die Alphonsinischen Tafeln und die im Besitze der k.k. Hofbibliothek in Wien befindlichen Handschriften derselben, *Sitzungsberichte der Mathematisch-Naturwissenschaftlichen Classe der kaiserlichen Akademie der Wissenschaften* **102**, ii, a: 99–117, 1893.

[6] The abbreviation BN will henceforth be employed for the Bibliothèque Nationale, Paris, and its Latin MSS.

[7] Thorndike, Lynn, Dates in intellectual history: The fourteenth century, Supp. No. 1, *Jour. Hist. of Ideas,* 1945.

saw Profatius Judaeus revise his treatise on the New Quadrant, while the next year was marked by correction of the positions of the stars made at Barcelona with two great armillaries; the year 1303, by the composition of a treatise on the solid sphere by either Accursius of Parma or John of Harlebeke; the year 1304, by observation of the altitude of the sun at Florence with the quadrant; and so on for well-nigh every year of the century.

Nor does the present paper repeat anything from the volume on the *Sphere* published last year,[8] nor from that which will appear this year upon the literature in Latin on comets from 1238 to 1368.[9]

PREVIOUS PUBLICATION THEREOF

I would, however, like to repeat the titles at least of a few recent articles and notes in *Isis*, since they bear directly upon our topic of Pre-Copernican Astronomical Activity. One deals with astronomical observations made at Paris during the years from 1312 to 1315 [10]; another, with a work on the theory of the planets by Peter of Modena, written in 1342 or earlier [11]; a third, with the astronomical Tables of Jacob Poel at Perpignan in 1361 [12]; a fourth, with a Canon on Tables of the Conjunction and Opposition of Sun and Moon, composed by Nicholaus de Heybech of Erfurt in 1384 or 1394 [13]; the fifth and last, with the treatise of Thomas Werkwoth on the motion of the eighth sphere of the fixed stars, written in the year, 1395.[14] Thus in these five cases we see activity in as many different regions: northern France and southern France, Italy, Germany, and England.

These and the publications previously mentioned, since they are concerned for the greater part with the fourteenth century, might seem to reduce the chances of finding further material in the manuscripts for that period as against the probability for the fifteenth century. To a considerable extent this likelihood proved to be the case, although, as I have frequently contended elsewhere, the fifteenth century seems to have been a time of scientific decline.

FIRST HALF OF THE FOURTEENTH CENTURY

At any rate, in the first half of the fourteenth century we have to note the translation from the Arabic of an *Almanach perpetuum* in 1307 "for finding the true places of the planets in the signs." [15] The year 1320 was that from which the astronomical Tables, compiled much later for Peter III of Aragon,[16] dated the beginning of his Era.[17] It was in this same year that a John Vimundus composed a treatise on the Disposition of the Celestial Bodies which opens a manuscript [18] in which it is followed by the better known Canons of 1322 of John de Lineriis.[19] In another manuscript of the fourteenth century, now found at Cambridge, England, is a work on Equations of the Planets which opens, "The time corresponding to the year 1329. . . ." [20] This seems to date it, and is the sole instance known to me of these opening words. In 1332, the same year in which John of Genoa wrote his Canons of Eclipses,[21] another John, of Montfort, drew up a Table to learn the movement of Sun and Moon "according to the equation of

[8] Thorndike, Lynn, *The Sphere of Sacrobosco and its Commentators*, Chicago, Univ. of Chicago Press, 1949.

[9] Thorndike, Lynn, *Latin treatises on comets between the years 1238 and 1368*, Chicago, Univ. of Chicago Press, 1950.

[10] Thorndike, Lynn, Astronomical observations at Paris from 1312 to 1315, *Isis* **38**: 200–205, 1948.

[11] Thorndike, Lynn, Peter of Modena, *Isis* **39**: 239, 1948.

[12] Thorndike, Lynn, Other astronomical tables beginning in the year 1361, *Isis* **34**: 6–7, 1942.

[13] Thorndike, Lynn, Nicholaus de Heybech of Erfurt, *Isis* **39**: 59–60, 1948.

[14] Thorndike, Lynn, Thomas Werkwoth on the motion of the eighth sphere, *Isis* **39**: 212–215, 1948.

[15] BN 7403, 14th century, fol. 1r—, "Incipit almanach perpetuum ad inveniendum vera loca planetarum in signis translatum de arabico in latinum anno domini M.CCC. VII. incompleto. Subtrahe igitur ab annis domini imperfectis 1306 et quod remanet vocatur residuum. . . ."

In a book which appeared since I read this paper Millás Vallicrosa, *Estudios sobre Azarquiel*, 353–354, 395–402, 1950, has discussed and published a part of this Almanach from another codex, Madrid 17,961, fols. 55r-91v. Since he alludes to it as the sole known MS (*ibid.*, 396, "a base del unico manuscrito conocido"), its occurrence in BN 7403 was evidently unknown to him.

[16] Thorndike, Lynn, Introduction and canon by Dalmatius to Tables of Barcelona for the years, 1361–1433 A.D., *Isis* **26**: 310–320, 1937. A supplementary article will soon appear in *Isis*.

[17] BN 10263, fol. 55r, bottom margin: Nota quod magister presupponit operantem scire eram regis Petri quando vult operari, que sic scitur videlicet minuendo ab annis Incarnationis annis 1320. Residuum est era regis Petri que incipit in mense Martii.

[18] BN 7286, 14th century, fols. 1r-8v: Dispositio corporum celestium continetur hic cum suis argumentis. Et primo sequitur de radice coniunctionum mediarum solis et lune . . ./. . . ad utilitatem scolarium universitatis Parisiensis et omnium aliorum.

[19] BN 7286, fols. 10r-41r.

[20] Peterhouse 75, fols. 1–50: Tempus correspondens an. 1329. . . .

[21] Concerning John of Genoa consult Duhem, *op. cit.* **4**: 74–75, and Thorndike, Lynn and Pearl Kibre, *A catalogue of incipits of medieval scientific writings in Latin*, col. 26, Mediaeval Acad. of America, 1937.

the Alfonsine Tables." [22] In the same year Wilhelmus Scotus, writing on conjunctions of the planets, added a note on a constellation of 1332.[23] Two years later the physician to another Alfonso, this time king of Portugal, and to his daughter Maria, who was queen of Castile and Leon, was engaged in astronomical translation at Seville. This physician was himself named Alfonsus Dionysus.[23a] In a manuscript at Basel there are Tables for the true movement of the sun during the years 1336, 1337, 1338, and 1339,[24] followed by a Table of Continuation of the true place of the sun for the years 1340–1456.[25] The year 1340 also figures astronomically in three different manuscripts of the Bibliothèque Nationale. One gives the names of the fixed stars whose positions were verified for that year by Henry Selder.[26] Another manuscript informs us that the fixed stars were verified in 1340 by certain famous masters and "great experiences." [27] The third takes the year 1340 as the point of departure for Tables of the Seven Planets for thirty-two years following.[28] John of Northampton designed an astronomical ring and perpetual calendar in 1348, and an anonymous tract concerning it is extant a half century later in 1394.[29]

SECOND HALF OF THE FOURTEENTH CENTURY

Passing on to the second half of the fourteenth century, after the catastrophic Black Death and while the Hundred Years War was well under way, we find in a manuscript at Berne astronomical Tables for the meridian of Paris for the year 1359 by a Hermann Stilus, a Saxon of Northem, Norchem, or Norhen.[30] Other Tables by the same man for 1361 follow in the same manuscript,[31] and in 1362 he equated the true positions of all the planets.[32] 1361 was a great year for Tables, since in addition to those to which attention has been already called elsewhere, of Barcelona by Peter III, Dalmatius and Jacob Carsoni, and of Perpignan by Jacob Poel, there are yet others by a Bonetus, preserved in a manuscript at Paris.[33] A manuscript at Utrecht of the fourteenth century contains material on a solar eclipse of 1366.[34] A Table of Mansions (of the Moon) and a Table of the Fixed Stars were both verified at Tournai in 1367.[35] In the next year we have both Tables and Canons on Tables for Paris, and we are told that they were composed there and were approved by various *astrologi* in 1368.[36] A Canon on the True Conjunctions and Eclipses of Sun and Moon in another manuscript, appeared to be acephalous, but it ran to the year 1386.[37] Eustachius de Eldris dedicated his work on Directions to Cesareus de Ghemert, physician to the duchess of Brabant, in 1390, completing the work in Liége on the sixth

[22] Bartoniek, Emma, *Codices latini medii aevi Budapest*, 1940: MS 62, fols. 45–46.

[23] Munich, Nationalbibliothek, cod. lat. 228, 14th century, fol. 220v; *cf.* Cues 209, fol. 76v.

[23a] BN 7316A, fols. 179va-180rb, opening and closing: "Celi enarrant gloriam dei et opera manuum eius annunciat firmamentum, sitque nomen domini benedictum qui mihi servo suo per suum angelum lucis secreta siderum revelavit . . ./. . . Explicit hoc opusculum translatum et perfectum et correctum 15ª die Marcii licet fuisse incoatum 11ª die anno domini 1334 apud civitatem Hyspalensem per magistrum Alfonsum Dyonisum clericum et medicum illustrissimi principis domini Alfonsi regis Portugalie et filie sue domine Marie Castelle et Legionis regine. Et nota quod non curavit translator de ornatu verborum sed de proprietate verborum interpretis, malens iuxta doctrinam et exemplum Boetii in De scolastica disciplina fide translatione deservire quam temeritatis arrogantia reprehendi. Explicit."

[24] Oeffentliche Bibliothek der Universität Basel, MS F.II.7, fols. 78r, 78v-79v.

[25] Basel F.II.7, fols. 80r-83r.

[26] BN 7277, fol. 2r: Nomina stellarum verificaturum ad annum domini nostri Ihesu Christi 1340 per Henricum Selder. Opposite one or two items is written: Hoc non est de Henrico Selder; non sunt de Henrico.

[27] BN 7292, fol. 41v.

[28] BN 10262, 15th century, fols. 9r-46v: Tabula porcionis solis ad annos radicum et perpetuacionis et ad menses dies horas et fracciones. Radices ad 32 annos post annum 1340 . . ./. . . Expliciunt tabule de septem planetis et de veris locis eius (*sic*).

[29] British Museum, Royal MS 12.E.XVIII., 40 fols. There is a figure of the ring at fol. 39. The tract opens, Quamvis celi terreque conditor et siderum rector . . . , and A. G. Little, *Initia operum latinorum quae saeculis XIII, XIV, XV attribuuntur*, Manchester, 1904, suggests the name of Richard Maidstone as its author. But in the 1921 *Catalogue of Western Manuscripts in the Old Royal and King's Collections*, by Sir George F. Warner and J. P. Gilson, the tract is still represented as anonymous.

[30] Stadt und Hochschulbibliothek, Bern, MS 454, fols. 65r-93v. In Metz 287, however, according to the catalogue of the MSS there, the date is given as 1349 rather than 1359.

[31] Berne 454, fol. 65v.

[32] Berne 454, fol. 89r.

[33] Tabule Boneti: BN 7287, fols. 108rb-112va.

[34] Utrecht 317, fols. 103–105v, 106r, 107r.

[35] BN 7277, fol. 1v, Tabula mansionum verificata Tornaci anno domini 1367 completo; fol. 3r, Tabula stellarum fixarum verificata Tornaci anno domini 1367 completo; fol. 5r, Tabula stellarum fixarum secundum quod in ymaginibus signorum, primo pro tempore Ptholomei, secundo pro tempore Alfonsi, tertio pro anno Christi 1367 completo.

[36] BN 7287, fols. 87rb-92rb. I have a photostat copy of these leaves.

[37] BN 15171, fol. 117r, Explicit Canon super coniunctionibus veris solis et lune eclipsibus usque ad annum domini 1386.

of March.[38] For the next year, 1391, there are astronomical Tables and a Calendar with astronomical data.[39] In 1392 a Nicholaus of Erfurt made a copy of the Alfonsine Tables which is preserved in a manuscript at Naples.[40] As the fourteenth century closed, Tables were composed for Saturn from 1400 to 1458, for Jupiter to 1482, for Mars to 1478, for Venus only to 1408, for Mercury to 1417, and for the Moon to 1445.[41] In another manuscript are Tables with roots throughout for Ferrara and Rome for the years 1400 and 1440.[42]

EARLY FIFTEENTH CENTURY

Proceeding in the fifteenth century and continuing to follow a roughly chronological order of presentation, we note astronomical Tables for the last four months of the year 1402.[43] These are in a manuscript at Paris. In another at Basel is a Table compiled in 1406 by Nicolaus de Raterstorf, a Dominican of that city.[44] A third codex at the Bibliothèque Nationale gives for the year 1407 the location of the fixed stars and their movement at the meridian of Paris.[45] In this manuscript the location of the fixed stars in 1410 is compared with their positions as given in the Alfonsine Tables.[46] It was such data that made the need of new astronomical Tables and even new astronomical theory increasingly felt. In one of the two manuscripts at Paris of which we have just been speaking are Ephemerides or planetary Tables for 1413,[47] while in the other are presented for the year 1420 the location of the North Star, a Table of the fixed stars, and a comparison of the movement of the eighth sphere with its movement at the time of the Alfonsine Tables.[48] Similarly in a manuscript at the Vatican are found Tables of the fixed stars verified about 1420 by Johannes de Wachenheim.[49] And in an Ashmole manuscript at the Bodleian, Oxford, are a verification of the positions of the fixed stars in 1428, and instructions for finding the ascensions of the signs for Newminster in Northumberland.[50] In yet another manuscript at the National Library in Vienna occurs the only copy known to me of a certain tract on the movements of the planets.[51] It is followed by Tables of solar and lunar eclipses during the years 1432–1449 and the days of the month when they were observed.

MID-FIFTEENTH CENTURY

On the inside of the front cover of a manuscript at Basel[52] it is recorded that in 1436 the vernal equinox occurred at Paris on March tenth, 17 hours, 27 minutes, 46 seconds, 30 thirds, and 50 fourths, while at Montpellier the time was 17 hours, 39 minutes, with the same number of seconds, thirds, and fourths as at Paris. In the next year, 1437, are dated the astronomical Tables of Melchion de Friquento of Naples, which are preserved in a Paris manuscript.[53] Another contains Tables of 1438–1439 for Sun and Moon, and Tables of 1438–1445 for the other five planets.[54] The Basel manuscript which was just mentioned has a marginal note correcting astronomical Tables in the case of the current year, 1444.[55] The name of Melchion de Friquento occurs again in another Paris manuscript where Canons for his Tables are dated 1445.[56] In that year, too, took place another

[38] BN 7279, fols. 1ra-4va. The dedicatory epistle opens, Variis scientiarum radiis illuminato . . . ; the text opens, Prima regula talis est . . . ; the work closes, . . . Hec dicta per me de directionibus meorum magistrorum submitto correctionibus cum ipsorum benevolentia. Completum est hoc opus anno a nativitate domini nostri Ihesu Christi 1390 incompleto mensis Martii die sexta in ipsa Leodiensi. This MS once belonged to Niccolò Leoniceno, humanist physician of the late fifteenth and early sixteenth centuries, as the words, Domini Nicolai Leoniceni, written on the fly leaf show.

[39] BN 15171, fols. 56v, 57r-62r.

[40] Naples VIII.D.31. This MS and date are not noted in my article listed above in note 13.

[41] BN 7403, fols. 40r-69r.

[42] Vatican Latin MS 3099, anno 1472, fols. 39r-51. Previously noticed by Fritz Saxl, *Verzeichnis astrologischer und mythologischer illustrierter Handschriften des lateinischene Mittelalters in römischen Bibliotheken*, Heidelberg, 1915.

[43] BN 7427, fols. 17r-18v.

[44] Basel F.VIII.16, fol. 53v.

[45] BN 7277, fols. 1v, 5v.

[46] BN 7277, fol. 4v.

[47] BN 7427, fols. 1v-7r.

[48] BN 7277, fols. 1v, 3r, 7r.

[49] Vatican Palatine Latin MS 1368, fols. 49-58v.

[50] Ashmole 191.II, fols. 138v-140r. I hope to publish this text in some periodical soon.

[51] Vienna 3528, fols. 171r-193v, opening and closing, Que stelle errantes dicuntur . . ./. . . lunares eclipses continere.

[52] Basel F.II.7.

[53] BN 10262,, fols. 46v-71r: Incipiunt tabule Frequentine pro coniunctionibus et oppositionibus solis et lune et eorum eclipsium quas ego Melchion de Friquento de Neapoli composui ad honorem et laudem sanctissimi trintatis et mei preclarissimi doctoris domini Antonelli de Laurencio de Auersis in anno domini 1437 completo . . . /. . . Expliciunt tabule Frequentine. Sit laus glorie Christe.

[54] BN 7427, fols. 8r-10v, 11r-16r.

[55] Basel F.II.7, fol. 80v.

[56] BN 10263, fols. 46r-50r, opening and closing: Canones super tabulas Frequentinas de coniunctionibus solis

verification of the fixed stars, the recording of which occupies nineteen leaves divided into columns for their longitude, latitude, magnitude, whether in the northern or southern hemisphere, and nature or association with one or two planets.[57] The *Tabule Boneti,* of which we spoke under the year 1361, forecast a conjunction and an eclipse for August 28, 1448.[58] In a manuscript at Berne is an astronomical figure for Tuesday, February 24, 1450 at 12.10 o'clock.[59]

NEW INSTRUMENTS

In this same year 1450, a Saracen Alfachy was said to have brought a new astronomical instrument called *Sexagenarium* from Cairo in Egypt to Valencia in Spain.[60] It was a time when there seem to have been frequent attempts to devise such new instruments. Ten years earlier a Venetian, Giovanni da Fontana, had completed an elaborate account, filling some 220 leaves of manuscript, of a triangular instrument—De trigono balistario—which he regarded as "most novel." [61] The second half of his long treatment dealt with the application of the instrument to the measurement of the heavens, followed by a Table of the fixed stars verified for that year, 1440.[62] About the same time Giovanni Bianchini of Ferrara [63] was devising a somewhat similar instrument to find the altitude of the stars. It consisted of two sticks of equal length fastened at their ends at right angles to each other. One stick was divided into 800 equal parts, and attached to the free end of the other was a swinging rod with two sights on it. If the stick with 800 divisions was placed perpendicular to the ground and the other stick parallel to the ground, one could see through the sights any star with an elevation of less than 45°, while to observe stars with an elevation of more than 45°, the stick with 800 divisions was held in air parallel to the ground and the other stick perpendicular to the ground with the sights at that free end. Yet another astronomical instrument was constructed by a Dr. Fuchs of Erfurt in 1452,[64] while an account of the *Sexagenarium* was translated from Arabic into the vernacular at Valencia in 1456.[65]

OTHER MANUSCRIPT DATA

Meanwhile, Bianchini had been observing lunar eclipses of February 16, 1440, September 12, 1448, July 13, 1451, and May 1, 1455. In a manuscript at Arras [66] which I have not yet examined the fifth treatise is an anonymous work on the Sphere, dedicated on November 9, 1450 from Monselice [67] to Domenico Malatesta who some years later founded the famous library at Cesena.[68] The second entry in the manuscript is Tables of the conjunction and opposition of sun and moon and their eclipses, ascribed to Bonetus and so presumably those of 1361 already mentioned, but here verified to the year 1455 and copied by Conrad Schoulter, M.D.,[69] in 1463. Zinner lists two other manuscripts of the fifteenth century at Munich and Vienna with the same incipit.[70] A single manuscript at Paris contains an Almanach and daily positions of the planets for the years 1454–1455, Tables for 1456, planetary positions for 1460, an Almanach for 1461, Tables of the planets for 1463, an Almanach of Paris of 1464, planetary Tables for the same year, and other Almanachs for 1465 and 1467.[71] In another Parisian codex are recorded conjunctions and eclipses for 1460, 1461, 1462, 1464, 1465, 1466, 1468, and 1469, with ac-

et lune et eorum eclipsium noviter compilate sive composite per Melchionem de Friquento de Neapoli tempore incarnationis Ihesu Christi 1445 ad meridianum civitatis Neapolis cuius latitudo est gradus 40, longitudo vero gradus 36 minuta 15 . . ./. . . Expliciunt canones tabularum Frequentinarum.

[57] BN 7287, fols. 126r-144r, with 3 pages allotted to the stars in Aries, 4 for those in Taurus, four plus for Gemini, 2 for Cancer, 3 for Leo, two plus for Virgo, three minus for Libra, four minus for Scorpio, three minus for Sagittarius, 3 for Capricorn, three plus for Aquarius, and two plus for Pisces.

[58] BN 7287, fol. 111vb.

[59] Berne 454, fol. 25r.

[60] BN 7416A, fols. 3r, 10v. I shall treat of this instrument more fully in an article which is to appear in *Isis.* Steinschneider held that Fachy was was not a proper name but equivalent to Faquin or learned physician.

[61] MS Canon. Misc. 47, Bodleian Library, Oxford. I have a reproduction of it.

[62] Canon. Misc. 47, fol. 221r-v.

[63] An article by me on Giovanni Bianchini in Five Paris Manuscripts is about to appear in *Scripta Mathematica.*

[64] BN 10263, fol. 168vb.

[65] BN 7416A, fol. 58v.

[66] Arras 688, 15th century. The fourth treatise, at fols. 60r-65v, is Peter of Abano on the motion of the eighth sphere.

[67] Ex oppido Montisilicii.

[68] In 1464: see the catalogue of Muccioli.

[69] De Retenburga (Rhätikon?).

[70] Ad faciendum horologium murale . . . ; E. Zinner, *Verzeichnis der astronomischen Handschriften des deutschen Kulturgebietes,* Munich, 1925, Nos. 9703, 9704; MSS Munich, cod. lat. 27, fol. 128; Vienna 5258, fols. 97r-98v.

[71] BN 7427, fols. 19r-38v, 58v-70r, 91r-96v, 97r-102v, 103r-113r, 114r-121v, 122r-127v, 128r-133v, 72r-90v.

companying figures.[72] One Vatican manuscript has Scholia to astronomical Tables made by Gabriel de Pirovano in the years 1438, 1467, and 1470[73]; another has additions to the Alfonsine Tables made by Giovanni de Calemonte, Professor at Perugia in 1467.[74]

SEXAGENARIUM AGAIN

Symon de Phares, writing at the close of the fifteenth century, under the year 1462 named a master Jehan du Locron as inventor of an astronomical instrument called *Solitarium*, and under the year 1464 a master Jehan de Bosma as maker of another instrument called *Sexagenaire*.[75] This is the *Sexagenarium* to which we have already referred, and in a manuscript which may have been the very one known to Symon,[76] a John de Bonia or Bonja—rather than Bosma or Bosnia—is variously represented as the translator of a work on it from the Arabic into Latin[77] or merely into the vernacular,[78] and as the author of a Latin work on it, completed in 1464 as Symon said, in which he claims to have greatly improved upon the original Arabic instrument.[79] Yet another work on the *Sexagenarium* is credited in another manuscript to a Cristianus de Proliano in 1475 or 1479.[80]

FURTHER MANUSCRIPTS

Our manuscripts have astronomical Tables for every year between 1470 and 1481 except 1473, and carry on for some points to 1484 for the meridians of Ferrara and Paris.[81] The Berne manuscript gives the astronomical position for noon on the last day of December, 1475 and the first of January, 1476.[82] For 1477 there is an astronomical note in one Parisian manuscript,[83] and Almanach Tables calculated by Nicolaus de Thutthovia, in another.[84] Tables of conjunctions and oppositions of Sun and Moon for the years 1482, 1483, 1484, and 1485 appear in a third codex at Paris.[85] The Berne manuscript has a figure for the entry of the Sun into Aries at the vernal equinox of 1487, and roots of the planets for the same year and the meridian of Paris.[86]

By this time the age of manuscripts had pretty much run its course, and the period of the printed book, to which we should have to look for further examples of astronomical activity, was well under way.

CONCLUSION

Our manuscripts have disclosed a number of new and unfamiliar names in the history of astronomy: John Vimundus, Alfonsus Dionysius, Henry Selder, Hermann Stilus, Bonetus, Eustachius de Eldris, Nicolaus de Raterstorf, Melchion de Friquento, Christianus de Proliano, John de Bonia, Nicolaus de Thutthovia. They have shown that translations from the Arabic and the composition of new treatises in Latin continued from the twelfth and thirteenth into the fourteenth and fifteenth centuries. The time of the vernal equinox was carefully observed; the positions of the fixed stars were frequently verified. New astronomical Tables were continually being drawn up, and new astronomical instruments were being devised. The men of the fourteenth and fifteenth centuries were astronomically minded, and the ground was being prepared for the harvest time of the Copernican theory.

[72] BN 10263, fol. 38r-v.

[73] Vatican Latin MS 8951.

[74] Vatican Latin MS 8174.

[75] Symon de Phares, *Recueil des plus celebres astrologues*, ed. Ernest Wickersheimer, 260, Paris, 1929.

[76] BN 7416A. In the sixteenth century it belonged to the French astrologer Mizauld or Mizaldus, and it contains a memorandum by Oronce Finé.

[77] BN 7416A, fol. 10v, ex arabico idiomate in Italien (*sic*), mox in Latinum a Ioanne Boniae Valentino.

[78] BN 7416A, fol. 57v, Incipit lectura Sexagenarii quam Iohannes Bonie ex arabico in ydioma Valentinum interpretatus est, et quidam in romanam linguam omnium preclarissimam nunc traduxit.

[79] BN 7416A, fol. 11r, Ad Ferrandum Valentinum virum insignem preclarumque iureconsultum Iohannis Bonie ad novam inventionem in sexagenarium instrumentum incipit prefatio; fol. 57r, Expletum fuit hoc opus per dominum Iohannem de Bonja Valentinensem 12 mensis Martii 1464.

[80] BN 10263, fols. 137r-147r.

[81] BN 7427, fols. 136r-303r.

[82] Berne 454, fol. 26r.

[83] BN 7287, fol. 144r.

[84] BN 7427, fols. 43r-55r.

[85] BN nouv. acq. 3034, fol. 305r.

[86] Berne 454, fols. 23r, 19r-v.

SOME RESULTS FROM DATA SECURED BY THE AMERICAN METEOR SOCIETY

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I. TELESCOPIC METEORS

Not long after the founding of the American Meteor Society in 1911, the idea came to me that a study of telescopic meteors on a larger scale than apparently before attempted might give interesting results. So, along with plans for encouraging the more scientific observation of ordinary meteors and fireballs, requests were made to both professional and amateur astronomers to let us have observations of all meteors seen when using any kind of telescope. Eventually a special printed form was devised and distributed to all A.M.S. members and to others who requested copies. Thanks to the help of about 81 individuals, and the more special efforts of about one half of them, I am able to present the results given in this paper. They specifically represent reports on 3,336 telescopic meteors, not counting a few very unusual "showers" which would not fit into more general statistical investigation. The period covered is 1928 to 1948 inclusive. Some results from 1949 are added.

The instruments used varied from field-glasses to a 27-inch refractor, the majority, however, being observed with telescopes of from 4 to 8 inches aperture. Some observers gave much more data than others, but the average report was quite complete as to the meteors themselves. For comparison of magnitude curves, results by the writer [1] on 796 telescopic meteors, by A. M. Bakharev [2] on 546 telescopic meteors, by J. D. Williams [3] on 659 meteors seen with binoculars, by Fletcher Watson [4] on 190 meteors observed in 1939 with a 4-inch telescope, and by S.L.Boothroyd [5] on 707 meteors, were added.

[1] Olivier, Charles P., Results derived from 8,600 meteor observations made during the years 1919–1925, *Publ. Leander McCormick Observatory* **5** (1) : 1–49, 1929. Referred to as M4.

[2] Bakharev, A. M., Telescopical meteors in the years 1937–39, *Ann. Tadjic Observatory* **2**: 71–89, 1941.

[3] Williams, J. D., Relative frequencies of meteor magnitudes, *Astron. Jour.* **48** (1110) : 100–103, 1939.

[4] Watson, Fletcher, A study of telescopic meteors, *Proc. Amer. Philos. Soc.* **81**: 493–504, 1939.

[5] Boothroyd, S. L., Telescopic observations of meteor velocities, *Harvard College Observatory Circ.* **390**: 1–12, 1939.

We have had enthusiastic help from several members of the American Association of Variable Star Observers though vain attempts were made to secure more general cooperation. However, with the reports at hand, far the largest mass of such data ever to be investigated is the basis for what follows. In nearly every case it will be necessary to consult the accompanying table or diagram to test the validity of the conclusions. The work of one man, Cyrus F. Fernald of Wilton, Maine, perforce receives special attention. This is because he has observed with one telescope at one place, from 1941 to 1949 inclusive, has carefully recorded every meteor seen in accordance with instructions, and further kept a full record of how many minutes per month he was looking through his telescope. For these reasons his work permits of an especially full analysis.

Under Flower Observatory is listed the observations of every member of our staff who ever recorded a telescopic meteor. It seems useless to give the individual names, except to state that B. S. Whitney and P. H. Taylor saw more than others. Most meteors were seen when the photometer, with a field of about 16′, was in use. From the Southern Hemisphere reports came from the La Plata Observatory, University of Michigan Observatory at Bloemfontein, South Africa, and the amateurs in New Zealand and Australia. Most of the latter reports were prepared and sent to me by R. A. McIntosh, who is head of the Meteor Section in New Zealand. We also had one very active observer in Japan, Hideo Inouyi, and one in India, namely R. G. Chandra, both of whom sent in large reports. There are relatively few from Europe as the A.M.S. encourages amateurs there to report to their local societies. The rest came from amateurs in the United States, with the exception of a very few from professional astronomers. Indeed without cooperation from many people a paper such as this could not be prepared.

Figure 1 shows five curves giving the percentages of meteors of each successive whole magnitude seen. The first two curves refer to data first published here, the second two to data published

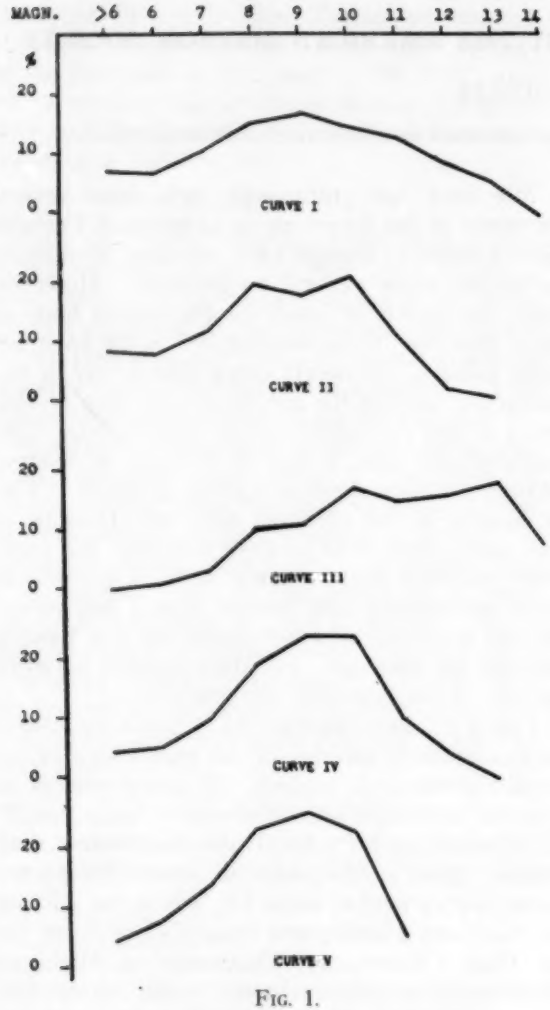

FIG. 1.

by me in M4, and the fifth to the Tadjic results, already referred to. Curve 1 is based upon 949 meteors with recorded magnitudes by 11 observers, seen with telescopes of from 13 to 27 inches aperture. A few seen with the finders of these telescopes are included. Curve II is based upon 2,125 meteors with recorded magnitudes by 25 observers seen with medium telescopes down to binoculars, though there are few of the latter. Curve III is based upon 278 meteors, seen before 1928 with the Virginia 26.3- and the Naval 26-inch telescopes. Again a few seen with their finders are included. Curve IV is based upon 518 meteors, seen before 1928, with smaller instruments. Curve V is based upon 518 meteors, seen at the Tadjic Observatory, with a comet seeker or binoculars. Observations by about 45 observers who sent in less than 15 meteors each, and which number 228, are omitted as being too lacking in homogeneity to be of value in this particular discussion.

The outstanding and wholly unexpected feature of all curves, but particularly of I and III which are most vital to the discussion, is that there is no appreciable increase in numbers below 10 magnitude, and that, to all intents, the curve becomes a straight line until the factor of faintness makes it fall rapidly. (Experience shows that for a given telescope the limiting magnitude for telescopic meteors is usually about $n - 2$, where n is the theoretical stellar limit). Exactly a similar phenomenon is shown by the other three curves, but here, as is to be expected, the maximum is shifted slightly to the left or brighter magnitudes. The only explanation that I can suggest is that, for causes not clear, particles which produce meteors fainter than magnitude 10 do not increase greatly in number as we go to fainter magnitudes. As we have five curves, from wholly independent sources and covering fully four decades, all indicating the same result, it is difficult to explain it by errors of observation.

As this result does not confirm two other studies made with the deliberate purpose of securing meteor magnitudes and by experienced observers, these papers were examined in detail. Figure 2 has four curves, showing the percentages in half-magnitudes, as the observers gave such de-

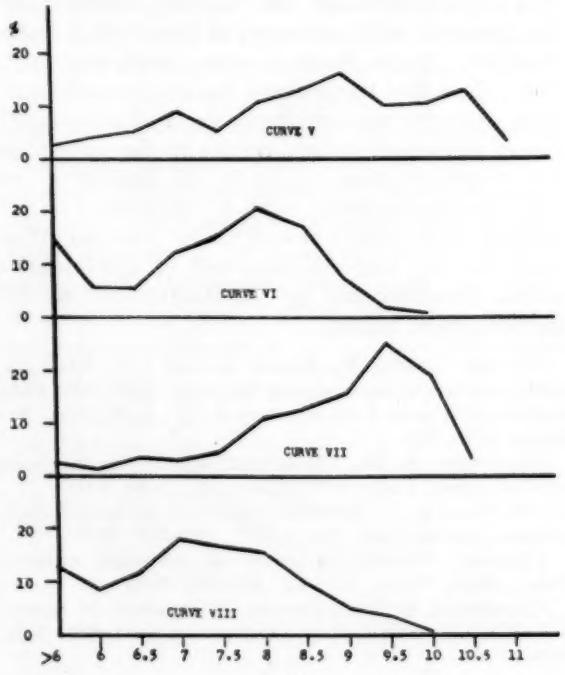

FIG. 2.

tailed data. Curve V is here redrawn to the half instead of whole magnitude scale as it is in figure 1, for comparison sake. Curve VI is based upon the paper by J. D. Williams in which he discusses 659 meteors seen with binoculars in the period 1935 to 1936. Curve VII is based upon the 190 meteors by Fletcher Watson seen with a 4-inch telescope in 1934 on 17 nights in July and August only. To draw curve VII, I have added 5.5 magnitudes to the results given by Watson in his table VII so that his results could be compared. This is, I believe, the reverse procedure used by him to derive his table. Curve VIII is based upon 707 meteors observed by S. L. Boothroyd.

The data are contained in Harvard Observatory Circular No. 390, which at first had been considered hardly comparable owing to the different way in which the 707 observations were made—namely by the rocking-mirror method. The corrections which had to be added to the observed magnitudes,

TABLE 1

Total	<6	6	7	8	9	10	11	12	13	14	—	Observer	Teles.
189	4	2	11	13	31	21	47	30	28	1	1	Armfield, L.	8,10,13
109	1	2	3	6	19	41	24	12	1			Chandra, R. G.	3, 6
123	8	20	15	24	26	18	9	1	2			Dartayet, M.	8, 17
592	87	54	33	89	69	145	103	11			1	Fernald, C. F.	8
255	17	13	24	37	50	47	26	23	7	4	7	Flower Obs.	18, 5
191	20	37	60	47	12	2	1	1			11	Inouyi, H.	1.1
106	7	8	14	10	14	13	7	10	11	3	9	Jessup, M. K.	27, 4
155	12	10	16	21	31	33	27	4			1	Jones, A.	5
183	4	4	23	64	48	32	7	1				Jones, F. H.	6
107	1	4	14	16	34	30	8					Kirkpatrick, G. P.	3, 4
172	1	11	8	32	47	52	20	1				Peltier, L. C.	6
2,182	162	165	221	359	381	434	279	94	49	8	30		
76	5	4	3	10	5	13	13	9	8	6		Dawson, B. H.	17
59	2	4	2	15	16	12	6	2				De Roy, F.	8
50	10	7	11	7	12	3						Ford, C. B.	3.5
49	1	4	8	12	11	11					2	Geddes, M.	5
58	4	2	9	13	8	10	9	1	1		1	Halbach, E. A.	13
87	1	3	10	19	16	16	15	5	2			Marsh, F. H.	8, 2
71	6	7	12	10	7	9	6	5	9			Preucil, F.	6, 16
56	2	3	12	16	14	9						Smith, F.	3, 5
42	4		3	8	8	8	7	2	2			Whitney, B. S.	10
548	35	34	70	110	97	91	56	24	22	6	3		
17	6	7		2	1		1					Ashurst, J.	9
25	3	2	4	12	4							Bateson, F. M.	3.5, 4, 9
23		1	2	6	4	3	4	3				Bouton, T. C. H.	7
20	1	2	7	6	2		1	1				Buckstaff, R. N.	12.2
28		2	2	8	5	8	3					Collum, Wm.	8
24	2	1	1	2	3	12	3					Cook, A. F.	2.5
19	2		3	1	2	5	4	2				Dermal, A.	4
30	3	1	1	10	11	3	1					Donner, H. F.	27, 4
18		1	2	2	3	4	2	1	2	1		Herbig, G.	8
37	8	5	9	5		5	3	1	1			Houston, W.	4, 6, 8
24	10	2	3	3	1	2	3					Logan, J. H.	11
16	2	2	1	4	3	2		1			1	McIntosh, R. A.	2.2, 14
29		3	8	7	6	5						Morshead, F.	6
15	6	5		1	2	1						Rosebrugh, D. W.	6
17			2	5	6	2	2					Shinkfield, R. C.	5
36	6		7	10	8	3	2					Smith, G. S.	2.5
378	49	34	52	84	61	55	29	9	3	1	1		
228	38	14	38	50	32	25	10	1			20	Others	
3,336	284	247	381	603	571	605	374	128	74	15	54	Total	

are very large and also remarks by the author himself indicated that he had included at least some objects as meteors of which he was uncertain. His observations were analysed in the same way as the Tadjic, namely all meteors from magnitude 6.0 to 6.4 were counted as 6, all from 6.5 to 6.9 as 6.5, etc. This permits them to be plotted by half magnitudes, and the result is curve VIII. This curve again does not resemble curves V to VII. However, frankly I do not think high confidence can be placed in the magnitudes on which it is based, owing to the very large corrections to which they were subjected, valuable as the data doubtless are for the purposes to which they were applied by Boothroyd. If other extensive lists of telescopic meteors exist, which are readily comparable with the ones already mentioned, I am unaware of them.

It will be seen at once that the sharp maximum shown in VII is totally lacking in curves I to IV inclusive, and indeed is much less clearly indicated in V, VI and VIII.

Indeed there seems far less similarity between curves V to VIII than between I and IV. In fact, I doubt that a person, who was not told in advance that curves V to VIII were supposed to show the same phenomenon, would ever guess that they had any common relations. Frankly, this dissimilarity makes me seriously wonder whether the extensive mathematical analyses in the original papers, to which the data forming the basis of curves VI and VIII were subjected, were entirely justified, and whether the far reaching conclusions as to fainter magnitudes are wholly valid. I do not question the thorough analyses, the observers' skill, or the excellent plans for the work, but I do question whether the data were extensive enough or representative enough to give anything like definitive answers. In any case, the new data here presented, just as handed over by the observers, in no case bear out the conclusion that, below magnitude 9 or 10, meteors of fainter magnitude grow more numerous by factors of from 2 to 4 per magnitude.

Table 1 gives all the 3,336 meteors, the observers being divided into four groups. The first are those 11 who observed more than 100 each; the second those 9 who observed from 40 to 100 each; the third those 16 who observed from 15 to 39 each; the fourth simply combines the observations of all others, 45 in number, without mention of names, who sent in less than 15 meteors. The first column gives the total for each observer, then follows columns giving the recorded magnitude,

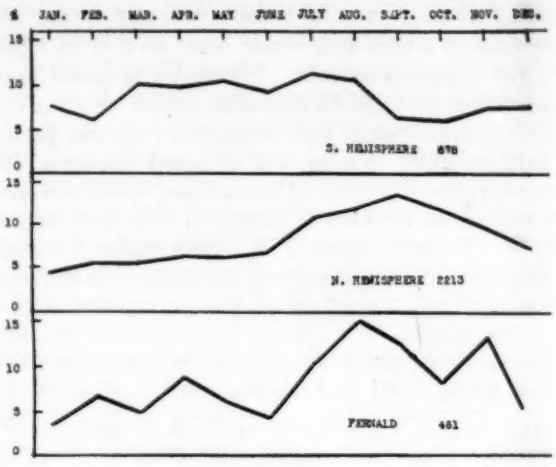

FIG. 3.

then the observer's name and aperture of telescope generally used or, in a few cases, the largest used. The data in this table form the basis for figure 1.

Figure 3 shows as follows: curve I, for Southern Hemisphere (all observers and 678 meteors) the percentage of meteors seen each month; curve II, for Northern Hemisphere (25 observers and 2,213 meteors) the percentage of meteors seen each month; curve III, for C. F. Fernald's work only, 461 meteors of less than 6 magnitude, also gives the monthly percentage. The data for III also are included in II.

In the case of Fernald's work a special analysis was justified thanks to its amount, continuity, and excellence. In particular it was further studied making the following assumption which was probably statistically justified, namely that, since so many telescopic meteors have very short paths and are hence near their radiants, we may assume that the direction of the telescope at the moment a meteor is seen points to the radiant itself. To justify this further, 119 meteors of 6 magnitude and brighter were omitted, as obviously such an assumption would not be justified for them. Using then 383 cases through 1948 inclusive we calculated the distance d of the radiant from the meteoric apex and the angle A which the direction of the motion of the meteor made with the arc joining radiant to apex. Both must have a maximum value of 180°. Curves were then plotted with d taken for every 5°, and with A for every 10°. This is figure 4. The curves show less, with certainty, than had been hoped. For d a maximum is indicated between 100° and 130°. For A indeed beyond 90° the curve is considerably higher, showing more meteors (actually 220 to

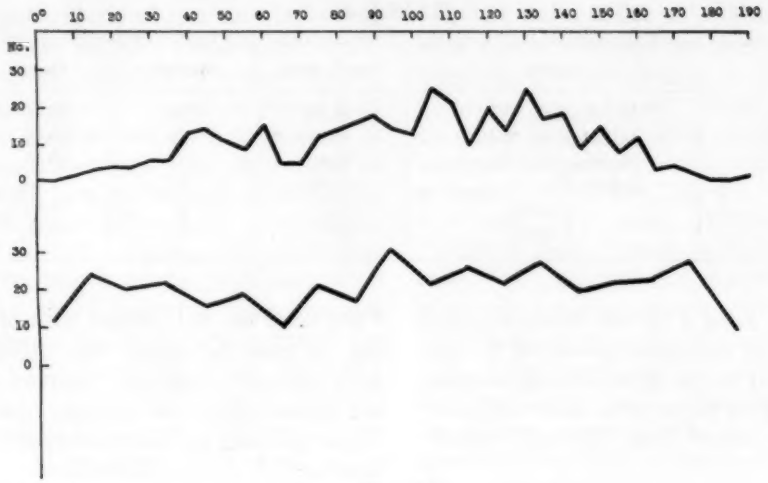

FIG. 4.

162) moving away from than towards the apex. As the apparent paths are all really projections, there is room here for an adequate explanation.

Taking the dates of the 8 chief annual showers and their combined duration as being 60 days, we find that Fernald observed (1941–1948) 383 meteors less than 6 magn. throughout the year, and 91 of these on the 60 days mentioned as shower dates. Of these 91 the position angles of the tracks showed that, at most, 31 could belong to the shower in momentary activity, while 60 could not, and were hence sporadic. Going therefore only by the date, this would mean 1.00 per day for the 60 days, while for the whole year (omitting the assumed 31 shower meteors) we get 0.97 per day. The excellent agreement of these two numbers would indicate that our number, namely 31, calculated as shower meteors, might be about correct.

TABLE 2

HOURLY NUMBER OF METEORS PER MONTH FROM FERNALD'S OBSERVATIONS

Month	Total	<6 Magn.	>6 Magn.
Jan.	0.32	0.24	0.08
Feb.	0.56	0.50	0.06
Mar.	0.32	0.28	0.04
Apr.	0.50	0.45	0.04
May	0.45	0.33	0.12
Jun.	0.32	0.19	0.13
Jul.	0.54	0.39	0.15
Aug.	0.52	0.39	0.13
Sep.	0.46	0.36	0.09
Oct.	0.35	0.26	0.09
Nov.	0.66	0.52	0.14
Dec.	0.35	0.28	0.07
Mean:	0.45±.09	0.35±.09	0.10±.03
Based on:	592	461	131 Meteors

TABLE 3

HOURLY NUMBER OF METEORS EACH YEAR FROM FERNALD'S OBSERVATIONS

Year	All meteors	<6 Magn.
1941	0.33	0.23
1942	0.39	0.33
1943	0.41	0.38
1944	0.43	0.33
1945	0.34	0.21
1946	0.56	0.47
1947	0.39	0.25
1948	0.61	0.53
1949	0.69	0.60
Mean:	0.46±.10	0.37±0.11

Table 2 shows, for Fernald, the hourly number of all meteors (592) and also of those less than 6 magn. (461) for each month of the year, along with their averages and deviations. Table 3 shows, for Fernald, the same data for each year from 1941 to 1949 inclusive.

Table 4 which follows gives data, based on reports of a few observers, showing the hourly number of meteors seen. Only when an observer could and did give a fairly accurate estimate of the actual time spent at the eyepiece of his telescope will his name appear here.

The first three were observing for other things and the meteors recorded were casually seen; the last two were observing deliberately for meteors only. Preucil used a 6-inch telescope with a $3\frac{1}{2}°$ field. Inouyi's observations were made during the

TABLE 4

Name	Station	Hourly Rate	Meteors	Dates	Notes
Armfield, L. E.	Milwaukee, Wis.	0.36	144	1934–5	casual
Halbach, H. A.	Milwaukee, Wis.	0.43	10	1935	casual
Houston, W.	Manhattan, Kan.	0.60	22	1939	casual
Preucil, F.	Joliet, Ill.	1.94	32	1935	planned
Inouyi, H.	Japan	11.2	67	1936	planned
Inouyi, H.	Japan	13.4	75	1937	planned

Perseid epoch only, using a 26 mm telescope, with a field of 8+° and an estimated power of 5. Attention may be called to the quite close agreement of the mean of the first three, who observed casually, with that determined from Fernald's observations.

RADIANTS FROM TELESCOPIC METEORS

Several observers sent maps from which it was possible to deduce 14 radiants; the other 6 come from given coordinates. Unfortunately all were not careful to give the exact epoch of the map they used and we have, from notes they give and other evidence, to try to determine this important point. The importance is evident as in many cases the radiant can be determined quite accurately to 0.1° and hence, for the result to be really useful, the epoch must be known. 1900 seems to fit most of the observers except Inouyi; he states that he used "Beyer's Atlas" which is of epoch 1855. Ford used the B.D. maps, also of 1855. In passing, it must be stated that others sent in maps with several telescopic meteors traced thereon, but from which no radiant could be deduced. These radiants are not corrected for zenith attraction nor for diurnal aberration.

While many observers sometimes gave the color of a telescopic meteor, most did not as this was a datum not specifically asked for on the record sheet. Franklin Marsh, working with the 8-inch refractor, Hood Observatory, Maryland, was most careful in this detail. Out of the total of 87 seen he recorded colors for 81 as follows: white 44, yellow 19, green 9, blue 4, red 3, orange 2. When he gave a combination as white-blue for instance, it is counted as of the first color mentioned. He also states that 3 paths were curved. M. K. Jessup, who recorded 106 meteors seen with the Lamont-Hussey 27-inch refractor and its finder during 1929–1930 (but who states he did *not* re-

TABLE 5

A.M.S. No.	Date (G.M.T.)	Meteors	Radiant		Observer	Acc.	Epoch
			α	δ			
3203	1931 Aug. 10.80	3	40°.8	+54°.4	Ford, C. B.	f.g.	1855
3201	1928 Nov. 11.57	11	10.	+40.	Smith, F.		1900
3204	1932 Dec. 10.61	1	294.6	+49.4	O'Byrne, S.	sta.	1900
3208	1934 Dec. 3.52	4	278.8	+ 8.5	Simpson, J. W.	v.g.	1900
3219	1937 Aug. 11.76	3	41.5	+56.2	Pruett, J. H.	f.	1900
3220	1939 Oct. 18.85	27	86.	− 5.2	Houston, S.	f.	1900
3212	1936 Jul. 29.2±	5	30.8	+54.8	Inouyi, H.	v.g.	1855
3213	1936 Aug. 11.12	7	46.7	+57.1	Inouyi, H.	v.g.	1855
3214	1936 Aug. 12.16	9	48.0	+57.7	Inouyi, H.	v.g.	1855
3215	1936 Aug. 13.14	8	49.5	+58.1	Inouyi, H.	g.	1855
3216	1936 Aug. 15.18	7	52.8	+59.0	Inouyi, H.	g.	1855
3217	1936 Aug. 17.16	9	56.2	+59.9	Inouyi, H.	v.g.	1855
3218	1936 Sep. 21.15	1	30.	+87.	Inouyi, H.	f.	1855
3207	1934 Aug. 12.83	7	41.2	+56.6	Armsfield, L. E.	g.	1900
3209	1935 Apr. 20.85	1	303.7	+31.1	Armsfield, L. E.	13–9m, Sta.	1900
3218	1936 Jul. 5.74	3	346.9	+59.7	Armsfield, L. E.	v.g.	1900
3211	1936 Jul. 5.83	4	295.8	+49.0	Armsfield, L. E.	v.g.	1900
3202	1930 May 11.5	1	109.2	−59.6	Jessup, M. K.	7m, Sta.	1875
3206	1934 Aug. 12.71	7	42.8	+55.1	Ford, C. B.	v.g.	1855
3205	1933 Nov. 22.92	1	0.0	−76.5	Geddes, M.	8m, Sta.	1900

cord all he saw, particularly of the very faint meteors), observed colors for 57. These were: white 16, yellow 15, red 13, blue 7, and orange 6; also there were 2 curved paths. It will be noted that white and yellow meteors are most numerous in the opinion of both observers. In the results of others curved paths are mentioned occasionally, but most did not make extra notes consistently enough to quote figures. Again some observers, at times, gave the length of the path: many so noted were from 5′ to 15′ long, but again the data are too incomplete to quote, though perhaps one or two hundred scattered cases could be found.

We have no case of a really persistent or long-enduring train, two of 10 seconds being the longest duration noted. This is rather unexpected, as we have so many cases, but evidently proves the telescopic meteors were mostly entirely destroyed at heights greater than the zone in which such trains usually are formed.[6] A few of the most interesting cases for the telescopic meteors will be quoted in the observer's own words: Fernald, 1946 Sept. 26, $\theta = 19^h\ 55^m$ at $\alpha = 15^h\ 44^m$, $\delta = +28°$, magn. −3, "Probably a fireball. Made a trail about 10′ wide which persisted for about 3 seconds." 1947 June 20, $\theta = 18^h\ 50^m$, $\alpha = 23^h\ 33^m$, $\delta = +35°$, "Did not see meteor. Noted smoke trail when checking ST And. Trail about 10′ long, to north west of ST. Very wavy. Lasted for about 10 seconds." 1947 Oct. 21, $\theta = 4^h\ 00^m$, $\alpha = 5^h\ 30^m$, $\delta = +68°$, +1 magn., Orionid very probably. "Trail 10 sec." Jessup, 1930 May 11 (1930.358) $\alpha = 7^h\ 17^m$, $\delta = 59°\ 37'$, epoch 1875: "Apparently a meteor coming straight towards the telescope. Brightness increased to at least 7.5 magn. perhaps 7.0. Blue center and red fringe. Total duration not more than ½ sec. Flash increased in size and brightness very quickly and as quickly disappeared. Blue light probably disappeared first . . . diameter about 10″ or 12″. Total diameter at maximum 30″±. Image resembled that of a bright star seen visually with a large photographic refractor. I assume the red fringe to be the trail of the meteor." (This is A.M.S. radiant No. 3202.)

[6] A full discussion of such trains for naked-eye meteors will be found in my two papers: Long enduring meteor trains, *Proc. Amer. Philos. Soc.* **85**: 93–135, 1942; Long enduring meteor trains, second paper, *ibid.* **91**: 315–327, 1947; reissued as Flower Observatory Reprints No. 60 and 69, respectively.

CONCLUSION

Perhaps the greatest value of this study is that, for the first time, enough data have been collected to give a fair indication of the rates of telescopic meteors, so that the average casual observer—by this I mean a person observing for other objects

TABLE 6

PERCENTAGE PER MAGNITUDE

Observer	Meteors	>0	0	1	2	3	4	5	6	Notes
Anderson, P.	1506	3.7	1.2	4.5	7.2	14.8	25.6	33.7	9.3	
Ball	1227	0.2	2.2	8.4	13.9	22.2	22.0	26.0	5.1	See M4
Barnes	1127	6.6	3.0	15.5	27.8	26.3	14.6	5.4	0.8	1948 obs.
Bessey, Mrs.	874			9.6	39.7	44.9	5.0	0.8		See M3
Borden	1540	0.6	14.0	8.4	14.9	28.3	27.1	6.3	0.4	1940 obs.
Brooks, D.	2971	14.7	9.5	12.6	12.5	20.4	18.0	10.9	1.4	See M3
Bruseth	944	0.4	0.1	6.7	15.8	24.3	31.5	18.6	2.6	See M3
Dole, R. M.	1702	2.6	7.9	12.3	14.5	20.0	11.6	16.7	14.5	See M4
Khan, M.	9859	5.8	3.1	8.0	19.0	26.1	26.5	10.9	0.6	1937 to 1948; part 1949 obs.
Koep, J.	4026	2.7	8.2	8.5	18.0	20.6	18.4	23.6		See M3
Knowles, J. H.	804	0.1	2.4	6.7	15.9	19.0	20.3	31.2	4.4	1948 obs.
La Paz, L.	2617	1.0	2.2	8.0	14.8	23.3	25.8	21.8	3.1	See M3
Loreta, E.	3585	1.0	1.4	5.9	20.3	32.0	32.0	7.4		
Olivier, C. P.	9225	2.6	2.1	6.6	13.5	29.0	31.7	12.3	2.2	
Richardson, J. T.	415	1.7	10.4	11.8	15.7	15.7	19.3	25.1	0.5	1945 obs.
Scott, T.	2069	5.7	5.3	14.2	27.1	28.0	12.4	5.6	1.8	
Trudelle, P.	1781		8.9	15.4	12.9	17.5	21.7	23.5	0.1	See M3
Misc. 17	2137	2.6	3.9	13.6	18.6	20.8	22.7	14.4	3.3	See M4, page 5
Total	48409									
Average per cent		2.9	4.8	9.8	17.9	24.1	21.4	16.3	2.8	$\Sigma = 100.0$
Hoffmeister, C.	4478	0.7	0.5	1.5	5.8	22.7	41.0	21.6	6.2	$\Sigma = 100.0$

TABLE 7

PERCENTAGES PER HALF-MAGNITUDE

Observer	Meteors	>0	0	0.5	1	1.5	2	2.5	3	3.5	4	4.5	5	5.5	6
Hoffmeister, C.	4478	0.6	0.5	0.0	1.3	0.8	4.3	4.2	15.0	12.3	26.4	10.9	16.1	1.7	5.7
Hoffmeister, C.	1537	1.2	0.6	0.4	3.2	1.7	5.3	3.8	13.3	9.7	21.6	13.9	18.1	2.2	4.9
Hoffmeister, C.	4595	0.3	0.5	0.2	1.9	1.4	5.3	4.4	15.0	12.2	23.0	15.5	16.1	1.9	2.7
Total	10610														
Average per cent		0.7	0.5	0.2	2.1	1.3	5.0	4.1	14.4	11.4	23.7	13.4	16.8	1.9	4.4
Khan, Mohd. A. R.	9859	5.8	3.1	*	6.4	3.2	11.9	11.0	14.8	11.5	17.3	6.9	7.5	0.6	

or purposes than meteor observation—may know what to expect. It must be emphasized that a low power eyepiece, with large field, vastly increases the chances for seeing such objects. Those of us who perforce have to use higher powers and smaller fields of view see comparatively few.

I hope further that it will encourage others to keep records on telescopic meteors in the future so that, after a decade or two, these tables and curves can be fully checked and further conclusions drawn. Incidentally, I would welcome suggestions for further analysis of the over 3,400 observations now in our files. Finally, may I express my thanks and deepest appreciation for the unselfish aid given by all the observers whose work is the basis for everything contained in this paper. It is a matter of deep regret that several who contributed are no longer living, and hence will be unable to receive even this totally inadequate expression of my appreciation.

II. RATES OF NAKED-EYE METEORS

After completing Part I of this paper, it seemed desirable to add some discussion of the rates of naked-eye meteors, found in our records. For this purpose I already had tables in M1, M2, M3, and M4, covering some of the most active observers before 1928. To these I have added certain others, as will be seen in table 6, since in the last column are given the years during which the observations were made. It will be seen that 48,409 meteors form the basis of this table.

It must be clearly understood that this is but a fraction of the data in our files, which might bear upon the subject. Also, not all of the work of some observers mentioned has been included: for instance, my own and Dole's since 1928 have not been reduced for magnitudes, and the first would amount to over 1,700 meteors, but mostly observed on shower dates. The same remark would hold for Dole. But it is lack of assistance which is the primary cause for all not being reduced and included.

A glance at the table will show individual peculiarities. For instance, Mrs. Bessey evidently lumped all bright meteors under magnitude 1; D. Brooks certainly overestimated the magnitudes of the brighter, while Mrs. Bessey and E. Loreta record none of magnitude 6. However, by averaging the results of 17 named observers, and 17 others taken as a unit, accidental errors are probably fairly well eliminated. I did have to reduce Khan's results to whole magnitudes, for inclusion in this table, and such reductions are never completely satisfactory. They appear in their original form in table 7, where they are compared with those of Hoffmeister. Curves in figure 5 indicate the plotted results from these tables. Also, a table of ratios is given below, following some discussion. I admit being very doubtful whether these data indicate as high a ratio of increase as is generally assumed, though of course no corrections whatever have been applied, and practically any type of correction would increase such a ratio.

At the end of the A.M.S. results appear percentages by whole magnitudes of 4,478 meteors observed by Hoffmeister. These numbers are copied from his book *Die Meteore*, page 17. The same 4,478 appear also in table 7, where they are given to half magnitudes. These latter came in a

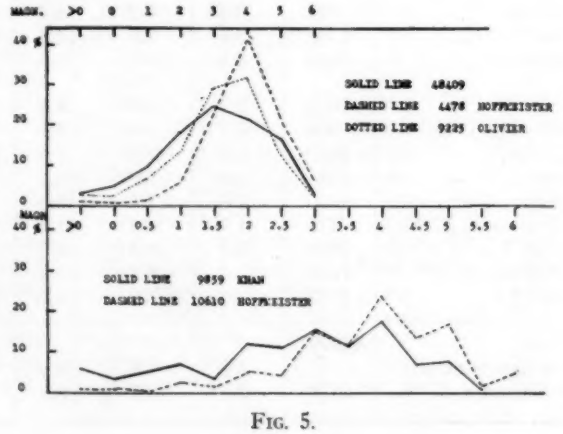

FIG. 5.

personal letter from him. If we compare the percentages of Hoffmeister, Khan, and Olivier, the observers with most meteors each and also most years of experience, we find considerable differences, Hoffmeister seeing more fainter meteors than the other two. This is largely attributable, I think, to his results being from nights of average activity when regular observations were made, wtihout casually seen bright meteors being included. On the contrary, Olivier's percentages include every bright meteor seen at any time, and further a quite large proportion of meteors from the chief showers. He, however, usually observed when the transparency was good. Khan's results, though more uniformly made all through the year, include many when he observed only from 15 to 45 minutes with poor transparency or moonlight. Perforce, these facts would give both a larger proportion of bright meteors than Hoffmeister, who presumably observed only on good nights for this particular series. Then, omitting meteors brighter than 0 magn. from the records of these three observers, we find the following ratios:

Magn.	0:1	1:2	2:3	3:4	4:5	5:6
Hoffmeister	1:2.95	1:3.85	1:3.89	1:1.81	1:0.53	1:0.29
Khan	1:2.58	1:2.37	1:1.37	1:1.02	1:0.41	1:0.06
Olivier	1:3.07	1:2.04	1:2.14	1:1.09	1:0.39	1:0.18

PERCENTAGES PER HALF MAGNITUDES

In table 7 are results by Hoffmeister and Khan. I entered the former in three lines, then took the straight mean of the percentages, so that the latter can be readily compared with those of Khan. Hoffmeister observed the 4,478 meteors at Sonneburg, Germany (latitude + 50°) from February, 1929 to March, 1931. The 1,537 and 4,595 meteors were observed in Southwest Africa (latitude − 22.6°) in 1937 from June to September. The 1,537 were observed from twilight to 10 P.M.; the 4,595 from 3 A.M. to morning twilight. There seems no significant difference in magnitude percentages between these two entirely homogeneous series. Khan's 9,859 meteors were observed from 1937 to June 1949 inclusive. In making up the tables of magnitudes the very few meteors recorded as + 0.5 magn. are included under 0 magn. A * in the + 0.5 column indicates this fact. Figure 5 will show how the results of these observers compare.

It has always been my personal opinion that any attempt to estimate accurately the brightness of meteors to half-magnitudes requires a skill almost too great to be attained. Hence, I have personally never tried to estimate closer than to whole magnitudes, and have advised others in the A.M.S. to do the same. Here, however, we have two of the most experienced observers in the world who have tried to get to the half-magnitude. Yet when we examine table 7 we see in practically every case, for both observers and even for the very brightest meteors, that the percentages in the columns which represent brightnesses between the whole magnitudes in adjacent columns are smaller than the next preceding. This is an obvious proof that, even for such skillful men, there is a tendency so strong to write the whole number rather than a fraction they cannot overcome it. It seems to me therefore that, even for them, it might have been better had they held to estimates for whole magnitudes. But whether others agree with me or not, in these cases, I think it quite clear that less trained observers would waste their time in trying to make estimates of magnitudes beyond their ability.

It is hoped that later we will be able to have all the data in our files reduced, after which a fuller analysis of rates would be justified. But it seemed that these preliminary results would be of considerable immediate interest, particularly when added to those on telescopic meteors in Part I. Hence they are presented.

It hardly needs comment that the A.M.S. records, probably the largest collection of meteoric data in the world, could and should be studied from other angles. While the number of published papers, based thereon, is already large, there remains material for many more. It may further be stated that we would gladly place the records at the disposal of any qualified person who would care to take up temporary residence near enough to use them at Flower Observatory, no matter what type of meteoric research he might be interested in.

WHAT SCHOOLS CAN DO TO ENCOURAGE THINKING

C. LESLIE CUSHMAN

Associate Superintendent, Philadelphia Public Schools

(*Read April 21, 1950*)

"MAN's truly great heritage is his brain."

"What can we do to make our children capable of thinking for themselves?"

The first statement is quoted from an address by George D. Stoddard, President of the University of Illinois.

The second is a question asked by Elvin C. Stakman, past President of the American Association for the Advancement of Science, at the 1949 meeting of that body.

The question of Mr. Stakman was asked because of his concern for the growing gap between the known world of the scientist and the limited comprehension of that world by the average citizen.

Modern man is in danger of becoming as ignorant of and overwhelmed by his environment as was primitive man. The basic problem is that we have millions of men who, seemingly trapped by technological and political machines, feel themselves individually lost and collectively incapable of managing their civilization. It is out of such conditions that totalitarian movements have arisen. Even within our own culture, we find a tendency continuously to drop to lower and lower levels the quality of the ideas communicated by press, radio, television, and conversation.

It is against such a background that the elementary and secondary schools of America today must consider the question of what they can do to make our children more capable of thinking for themselves. The assumptions of the past give no adequate answer. There is no evidence to show that increased years of school attendance for all children and youth will of itself provide a more thoughtful citizenry. Increased emphasis upon particular subjects of study gives no answer. The expedient of making school work more difficult seems to develop a dislike for intellectual activity. Finally, we may well include among the ineffective approaches, the procedure whereby institutions of one level dictate the programs of those below them—graduate schools the programs of undergraduate colleges, colleges the programs of high schools, high schools of junior high schools, junior high schools of elementary schools, and elementary schools of kindergartens. The dictate at this lowest level takes the form of the plea, "We wish you would at least teach children to sit still." None of these procedures offers even a partial solution for the problem of developing thoughtful young citizens.

This paper is a brief report of the tentative conclusions of a group of two hundred Philadelphia teachers and administrators who during the period of the past five years have consciously sought a more adequate answer to the problem of teaching pupils to think for themselves.

At this point it is fitting to acknowledge that these persons have been stimulated by the interest of a distinguished Philadelphian and member of the American Philosophical Society, Samuel S. Fels. In substance what Mr. Fels has said, and continues to say, is, "The longer I live the more evidence I find of men shutting their minds to new truth. This applies not only to persons with little schooling, but also to many who are supposedly well-educated."

Because Mr. Fels has an inquiring mind and believes in action, he has asked, "Isn't there something that schools can do, even with very young children, to lessen this tendency to the closed mind?"

The work of the teachers who have engaged in this Study constitutes no attempt to extend any frontier of knowledge. They have sought rather to develop for themselves a workable program, whereby their pupils will be taught to think more effectively. The conclusions they have drawn from their experiences are now being reported in a series of five guides, which they hope will be useful to other teachers here and elsewhere.

Their basic conclusion is that, if schools are to help pupils to learn to think, this must be a major concern throughout the entire program of each school. In other words, learning to think should be for both the teacher and the pupil a conscious

objective as the pupil proceeds from task to task, from the moment he enters the schoolyard in the morning until he leaves in the afternoon. It should be a conscious objective throughout the entire school life of every pupil from kindergarten to the completion of formal schooling.

Beginning with this assumption that learning to think should be a conscious objective in all school work, these teachers have asked, what practical measures can be taken by ordinary teachers in American schools that will contribute to this end. They propose the following four measures.

First, schools should provide the sort of classroom and school climate that will facilitate thinking; a friendly, informal, flexible environment, an atmosphere that invites the asking of honest questions, and that promises the likelihood of getting honest answers; a place where children have equal rights; above all, perhaps a place where no one need fear being laughed at, ridiculed, or censored for saying the wrong thing. Only in such an environment would Joe, a mature high school senior, express his honest belief that, "All politicians are grafters"; or Bob, annoyed because the student council's recommendation on a particular matter has been vetoed, blurt out, "That's the trouble. The teachers make all the important decisions around here."

A second measure they propose is that in every classroom increased attention should be given both by pupils and teachers to two related questions: who makes the decisions in the classroom; and, how are these decisions made? The most revealing single experience of many teachers in this study has been that of analyzing for a period of a day or two the source of the decisions concerning what is to be done in their classrooms. They have usually found that many decisions are made in terms of regulations outside of their control: by school regulations, regulations made by the principal, or regulations derived from tradition. Another large group is made by the teacher. A few are made cooperatively by the teacher and the pupils. And if the pupils are fortunate, they may have a chance to decide a few matters for themselves. This conventional pattern cannot be described as one favorable to the development of responsible and thoughtful citizens. It is, however, within the power of average teachers to do a great deal to improve the pattern in such ways as to promote cooperative thinking and living.

It has also been found that pupils can profitably learn to analyze the ways in which decisions are made. A six-point analysis, in no sense original, has seemed useful for this purpose. Relatively young children can learn to classify decisions as having been made on the basis of (1) tradition, (2) authority, (3) intuition, (4) what seems to be reasonable, (5) what seems to work, or (6) critical thinking. Through such analysis they may learn to look critically at the ways both they and their associates make up their minds and determine their actions.

The third measure follows naturally from the second. In every classroom particular attention should be given to the steps involved in good critical thinking: (1) the clarification of the problem, (2) the gathering of pertinent data, (3) the organization of the data, (4) the formulation of a tentative conclusion, and (5) the testing of that conclusion.

This conventional type of analysis can be applied with profit to many problems in every classroom, problems that range from such simple questions as, "How shall we enter the room?" or "Why does it take us so long to get started with our work?" to such vital issues as, "How can we reduce delinquency in our neighborhood?" or "How can we obtain better government in Philadelphia?"

In one classroom where the method of analyzing problems was frequently used, one girl was heard to exclaim, "Problems! problems! problems! All we do is talk about problems." A second girl's response that caught the ear of the teacher was, "Why are you complaining? Haven't you learned that thinking about problems is a good way of getting your work done?"

We may well wish that all of our youth could learn that lesson.

The fourth measure has to do with the building of values. In every classroom pupils should develop an awareness of the things they individually and collectively believe to be important—the ends that determine their action. They can profitably be shown how the values of different individuals vary, and they can acquire some understanding of the sources from which values are derived.

Teachers must of course learn that the acquisition of values is a very complex process. Children enter school with a highly complex pattern of values. These are not readily surrendered or exchanged for a new set that is more acceptable to the school.

However, in classrooms where there is the type of climate that has here been proposed, where

there is respect for the personality of every individual, where children share in making decisions, where they learn to think critically about problems that really matter both to them and to society —in such classrooms it is reasonable to believe that the values we treasure in a democracy will in varying degree become the values of children and youth.

The foregoing constitutes a very simple analysis of measures that the teachers who have shared in this study believe schools may employ successfully for the development of thoughtful citizens. Obviously, however, there is a great body of knowledge that might well be brought to bear upon each of the four measures, upon the cultivation of a classroom climate favorable to thinking; the consideration of the ways in which decisions are made; the learning of the processes of critical thinking; and the building of desirable human values.

Such merit as this program has rests on the simplicity of its structure. It removes the objective of teaching pupils to think from the vague classification of items that are believed to be inevitable concomitants of a good school. On the one hand, every teacher with the knowledge he now possesses can do something with respect to each of these four measures. On the other hand, as the thoughtful teacher seeks to apply these measures, he is challenged to develop a functional understanding of many fundamental concepts of psychology, sociology, anthropology, and of the insights of the philosopher and the man of letters.

In summary, this program challenges the teacher of 1950 to teach ever mindful of the initial assertion of this paper—that "Man's truly great heritage is his brain."

GREAT MEN AND SCIENTIFIC PROGRESS

EDWIN G. BORING

Professor of Psychology, Harvard University

(*Read April 21, 1950*)

INTRODUCTION

THE Great-Man theory of history is as old as history, as old as the kings who caused the records of their deeds to be cut in stone in order to let posterity know how it was that they had also carved out human destiny, as old as man's belief that he himself is a free agent who chooses his acts to shape his own life and the lives of those others whom his deeds affect. That makes this theory very old indeed, but its age does not render it obsolete. We still look to great men for the partial explanation of history, to Napoleon or Hitler, and we marvel that the little corporal or the paperhanger could so have changed the course of our civilization. In the smaller spheres of human affairs we acknowledge or seek out leaders, and the psychologist is told that the great need of the world now is for more leaders and better leadership. Certainly there are leaders and great men who play important roles in human affairs, and these persons are—practically by definition—those who greatly affect the lives of others. The Great-Man theory cannot be wrong. It expresses too obvious a truth about society.

Although the Great-Man theory cannot be wrong, since it is clear that men die having differed from one another in social effectiveness and therefore in greatness, there has been, nevertheless, for almost a century now, a growing suspicion that the theory asserts very little, since it specifies neither the attributes nor the conditions of greatness. What has been happening is that, as the new scientific age came on, philosophically minded persons began more and more to look for external causes of human action, abandoning free will as a positive explanation. Descartes (1650) had made it plausible to regard animals as automata but he had left man's rational soul free to choose what man should do. The French materialists and mechanists—writers like La Mettrie (1748) and Cabanis (1802)—extended the machine-theory to include man. In the nineteenth century, especially after Darwin's theory had made its great impress upon thinking, philosophers began to look, not merely for the attributes of greatness in order that they might understand its nature, but even more for the causes of greatness so that they might say when and why it occurs and perhaps take a hand in the production of genius. That is what Galton wanted to do.

Thus it comes about that there is a *personalistic* view of the operation of genius and a *naturalistic* one. The two are sometimes thought to contradict each other, but they are not really incompatible. The personalistic view is circumscribed. It exhibits man as a free originator, the genius as existing in his own right and solving problems more quickly or more correctly than the man in the street. The naturalistic view is broader and is capable of including the personalistic view. It shows genius, not only as the antecedent of new thought, but also as the consequence of its antecedent conditions. Thus the personalistic view preserves the dignity of man, for it leaves him a free agent accomplishing great deeds by the voluntary exercise of his capacities; whereas the naturalistic alternative robs man of this dignity by taking away his mystery and showing up his achievements and capacities as consequences of the conditions which create them. Science is always the great debunker because it reduces choice to necessity.

STATUS OF THE PROBLEM

Now let us see how this discussion has run, for not everyone has been so clear about it. Many have thought that there is an issue here, that the philosopher must choose between personalism and naturalism in assessing the role of genius in scientific progress.

The old view, as we have said, is the personalistic one. It was pointed up by Thomas Carlyle in his famous lectures of 1840 *On Heroes, Hero Worship and the Heroic in History*. "The history of what man has accomplished in this world," he wrote, "is at bottom the History of the Great Men who have worked here" (1840, lect. 1, par. 1). Carlyle's

paradigm for this hypothesis was his *History of Frederick the Great* (1865). Carlyle believed sincerely in the effectiveness of great men, and so he might, even though their greatness consists in their having been the agents of the inevitable.

Naturalism includes cultural determination, and we may note that the year 1859 is the date, not only for Charles Darwin's launching of his theory of evolution, but also for Karl Marx's *Zur Kritik der politischen Oekonomie*, the classical argument for the cultural determination of social history. More germane to our interest, however, is Count Tolstoy's depreciation of Napoleon a decade later in his great historical novel, *War and Peace* (1869), and his repeated return, late in that volume, to the theme of the adventitious cultural determination of history with the great men cast in the role of history's stooges. "A king is history's slave," he wrote. "History, that is, the unconscious, general, hive-life of mankind, uses every moment of the life of kings as a tool for its own purposes" (bk. 9, sect.1). "The higher a man stands on the social ladder, the more people he is connected with and the more power he has over others, the more evident is the predestination and the inevitability of his every action" (*ibid.*). So History, Tolstoy thought, is controlled by History and not by the Great Men. What is History? History is the aggregate of an infinitude of tiny events which make up, in Tolstoy's phrase, "the hive-life of mankind." The campaign of Napoleon in Russia in 1812 is to be understood as caused by a totality of little events, such as a whim of Napoleon on a certain day, the pique of Alexander on another day, the advice of a colonel who wished, not to advance History's Great Purpose, but to get for himself a promotion, and the fearless charge of the young cavalry commander who, unaware of danger, could not resist leading his men, without orders, in a gallop across the level plain. History, Tolstoy argued, reveals no Great Purpose in action. It is whatever the coincidences of many little events have made it and its Great Men are its distinguishing features. Just so you recognize a rhinoceros by its horn, a convenient label which the beast could nevertheless do without. Much more vital to the rhinoceros is his medulla oblongata, but that, since it does not show, is mentioned only by the biologist.

It was just at this same time that Francis Galton, influenced by the new theory of evolution, published his study of *Hereditary Genius* (1869). Galton believed that genius is biologically inherited and later he founded his gospel of eugenics upon this basic principle. His theory of genius was thus naturalistic although opposed to cultural determination. Galton held that genius will out, no matter what cultural inhibitors work against it, and that fame and reputation are therefore proper indices of the degree of genius. Galton was wrong, for culture plays an important role in determining achievement, but he helped to make usual the naturalistic interpretation of genius.

It was Herbert Spencer, the philosopher of evolution, who, while accepting Galton's notion of the inheritance of ability, maintained that social factors must also be taken into account. His basic postulate about society (and thus about History) was "that the character of the aggregate is determined by the character of the units" (1873: chap. 3, par. 1); yet he knew that the units also depend on the whole. It is not true, Spencer held, that genius will bloom no matter how poorly nourished. Beethoven, born of cannibal parents on a cannibal isle, would have achieved less than he actually did. At best his achievement would have been but relative to the current state of cannibal music. "Even were we to grant the absurd supposition that the genius of the great man does not depend on the antecedents furnished by the society he is born in," said Spencer, "there would still be the quite sufficient facts that he is powerless in the absence of material and mental accumulations which his society inherits from the past, and that he is powerless in the absence of the co-existing population, character, intelligence, and social arrangements" (1873: chap. 2, par. 13). Spencer was arguing for the multiple causation of achievements and events. Even if some one genius could have been a necessary cause for some particular historical event, he could hardly have been a sufficient cause. He himself would be, moreover, the consequence of his own antecedents, among which his ancestry would be but one of many items. An Eroica Symphony written for drums by a Fiji Beethoven would have differed greatly from the work which we know. It is thus clear that Tolstoy and Spencer were making the same argument.

This naturalistic interpretation of history was part of the nineteenth-century Zeitgeist. Personalism was on the way out then, although that fact was not clear until later. To Darwin's, Galton's, and Spencer's voices were added others. W. S. Jevons in his *Principles of Science* included a chapter on the character of the experimentalist, a chapter in which he attempted to reduce the discoverer to his attributes (1874: chap. 26). Grant Allen (1878) supported Spencer. William James

(1880) attacked Spencer and Grant Allen, who replied (1881). The historian, John Fiske (1881), also replied to James. Then James replied to them (1890).

William James was essentially a personalist. He never wholly accepted naturalism or experimentalism in his psychology. In the late 1870's he used Spencer as a text in Harvard courses because it gave his lectures point to have someone with whom he could disagree. Thus James dissented from Spencer's interpretation of genius, himself defending what Spencer had called "the Great-Man theory." Spencer had complained that the Great-Man theory was vague because it said nothing about the antecedents of genius nor did it in any way account for the occurrence of genius. James hurled this charge back. To hold that genius is the consequence of an enormous number of undetermined and indeterminable antecedents, he said, is vaguer than to recognize genius for what it is. James was coming to the rescue of the dignity of man. He wanted to save man from too much naturalism, and a naturalisticist, like a modern psychologist, cannot, of course, understand that.

In the present century we have had many studies of great men and of scientific eminence. Two outstanding books are Ostwald's *Grosse Männer* (1912), which deals in detail with the lives of six great scientists and considers the conditions of their productivity, and Lenard's *Grosse Naturforscher* (1929), which gives accounts of the lives of sixty-five scientists. These books border on personalism. They seek the conditions of genius, but they suggest that the achievement of genius is much more than the means by which the Zeitgeist realizes itself. More recently both Sidney Hook (1943) and Eric Bentley (1947) have re-created some of William James' nostalgia for a man of genius, formed in the image of his Maker and not manufactured by mere circumstance.

On the whole, naturalism has been gaining ground. An important paper was Kroeber's "The Superorganic" (1917) which, in one place, argued for the cultural determination of great achievement, argued by citing instances of simultaneous independent discoveries. If half a dozen persons invented telescopes in 1608 independently of each other, if Napier and Briggs both invented logarithms in 1614 anticipated by the little known Bürgi in 1611, if Newton and Leibnitz discovered the calculus independently in the 1670's anticipated by Isaac Barrow, if D'Alibard identified lightning with static electricity in 1752, just a month before Benjamin Franklin made his famous experiment, if both Priestley and Scheele discovered oxygen in 1774, may it not properly be argued that these discoveries depended, not primarily upon the attributes of the great men who made the discoveries, but rather upon the circumstances of the times, on the Zeitgeist, on Kroeber's "Superorganic"? The personalistic argument has been that the greatness of the man determines the greatness of the discovery. The naturalistic argument inverts the causal order; it is the greatness of the discovery which determines the greatness of the discoverer. In either case *Great Men make Great Discoveries*. Yet it may be that the Zeitgeist determines the Great Discovery and that he who makes the discovery, the Zeitgeist's agent, is Great merely because the times employed him. This theory reduces greatness from a cause to a symptom.

Kroeber's problem was tackled a little later by Ogburn and Thomas (1922). They published a list of one hundred forty-eight instances of two or more independent similar scientific discoveries or inventions, each pair or set of which were nearly synchronous. Their items were not always well documented and some appear to have been in error, but the total effect of the list is convincing. Discovery comes when the times are ready for it. Conant (1947) has noted that it can hardly come before, or that, if made early—as was the case with Mendel in 1870—there is apt to be no further advance so long as the inertia of the Zeitgeist hinders progress in that direction.

There has been of late a lot of talk about the psychology of the scientist and it is, of course, along naturalistic lines. Sometimes the problem of scientific originality is discussed and the frequency with which great discoveries are anticipated is noted (Boring, 1927). Idhe (1948) has reiterated the argument of Ogburn and Thomas. Sarton (1937), Conant (1947), and Bernard Cohen (1948) have all been discussing how scientific thought progresses, whether continuously or by spurts, how it is facilitated or retarded according as it is or is not in phase with the times, how a wrong theory can be replaced only by a better substitute and is not driven out by contradictory facts. This last is the *horror vacui* in scientific thought which Conant has stressed. And then, of course, the sociologists are full of talk about cultural determinations, which Toynbee (1933) has applied dramatically to the courses of civilizations. There is also Robert Warren's Penrose Lecture (1948) which suggests that the scientific age of

the last four centuries may at last be closing and that a new New Learning, a fresh Renaissance, may be ready to emerge. These discussions bear on the main issue but do not advance it far. Our problem is more intimate, for we seek to know what happens in the mind of the Great Man at the moment when he is being the agent of Science in the emergence of a Great Discovery or a Great Achievement.

But first we must consider how discoveries and inventions are related to each other and so to the times in which they occur.

ADVANCE OF KNOWLEDGE

The advance of scientific knowledge is a complex social affair. It involves knowledge previously accumulated, modes of thought which become easy when current, individual research sustained by motives of which the investigator himself is apt to be unaware. All this action and interaction becomes clear if we lay down here a number of propositions, clarifying them by citing historical instances. Here then is a table of nine conditions for scientific progress or its failure.

A discovery, an invention, an advance in knowledge is or may be. . . .

1. *Dependent upon previous discovery.*
2. *Dependent upon the availability of new methods or instruments.*
3. *A specification of what was previously indefinite or unsubstantiated.*
4. *Synchronous with similar discovery, yet independent of it.*
5. *Derived from unconscious communication.*
6. *Distorted by prestige or promotion.*
7. *Derived principally by insight.*
8. *Derived from an accidental occurrence.*
9. *Prevented from realization by the Zeitgeist.*

These are catch-phrases. Now we can expand each of them into more formal propositions.

1. *A discovery is seen to all concerned to be an outgrowth of previous discovery.* This is the rule for a growing science and it is the relationship about which the histories of science principally concern themselves. Science is serial. The next idea or discovery has to come before the next idea but one or before the next discovery but one. Given the invention of a boat and of a steam engine, it requires only an insight into a new relationship to invent a steamboat. Wundt made the new physiological psychology about 1862 out of British empiricism and experimental sense-physiology, but he had to have the ingredients before he could compose the blend, and each component had had its long history of development before it was ready for combination. It is these developments that Conant traces in his *On Understanding Science* (1947). There he shows the emergence and development of the concepts of the weight of the air and the spring of the air from Galileo in 1638 to Robert Boyle in 1666, and how Galvani's discovery in 1791 that a frog's leg will form the electrolyte of a battery if connected with two different metals led to the invention of the Voltaic pile in 1800 and the availability of direct currents thereafter. We need not labor this point. It is what everyone knows.

2. *Advance in knowledge is rapid when new methods or new instruments become available.* The building of telescopes in 1608 led to Galileo's constructing his in 1609 and his discovery of four of Jupiter's moons. That discovery upset theologically tempered science by showing that the Creator had exceeded the sacred number seven in his making of the celestial bodies, and later it provided the means for the measurement of the velocity of light. The improvement of the simple microscope in the latter part of the seventeenth century led to the discovery of bacteria and microorganisms, and the improvement of the compound microscope in the 1830's led to discovery of cell structure and the development of histology. Volta's invention of the battery, the invention of electromagnets and of the galvanometer, all in the early nineteenth century, made possible the study and measurement of the nerve impulse by du Bois-Reymond in the middle of the century and the invention of many other electrical instruments, like the chronograph and the chronoscope. The astronomers could measure absolute reaction times when they had these electrical devices. Similarly in the present century the development of the electronic tube has revolutionized, not only psychoacoustics, but also all sorts of laboratory work in physics, physiology, and psychology. This point too is obvious.

3. *Often a discovery or an advance consists in specifying what was formerly vague or in verifying what formerly rested on insufficient evidence.* This category cannot be rigorously applied to the particular case because it excludes those cases where the discoverer was unaware that he had been anticipated, the cases of the next two groups. That Newton's invention of the calculus was independent of Leibnitz's everyone agrees, but what about the contribution of Newton's teacher, Isaac

Barrow? Johannes Müller's doctrine of specific nerve energies of 1838 was anticipated in various ways by John Locke in 1690, Thomas Young in 1801, and Charles Bell in 1811. It is quite certain that Müller did not know about Bell, but what about the others? Perhaps this is a case of the Zeitgeist's carrying along through a century and a half. There are, however, some clear cases. Helmholtz in 1852 definitely did know Thomas Young's color theory of 1801, and Helmholtz made Young's theory important by developing it and giving it experimental stability. The Gestalt psychologists came ultimately to recognize their debt to von Ehrenfels' discussion of *Gestaltqualität* in 1890, though they never acknowledged William James' similar line of thought which also belongs to 1890. Köhler's doctrine of isomorphism between brain pattern and perceptual pattern has had a long history, and Köhler, when he first formulated his views, was aware of what G. E. Müller has said in 1896. In both of these cases the later formulation was clearer and more specific than the earlier ones and led to experimental programs and tests, but the progress of thought was nevertheless essentially continuous. There were no huge leaps.

4. *Two or more discoveries of the same fact are entirely, as far as can be determined, independent. They may be and often are synchronous.* These are the cases which constitute the chief evidence for the operation of the Zeitgeist, the "Superorganic." If Newton had not discovered and promoted the calculus, Leibnitz would have, or conversely, so runs this argument. If Bürgi failed to give logarithms to mathematics, there were both Napier and Briggs ready to do the Zeitgeist's bidding. The British speak of Boyle's Law, the French of Mariotte's. Legendre described the principle of least squares in 1806, Gauss in 1809, and both probably used the principle before they described it. Charles Bell in 1811 and Magendie in 1822 on the law of the spinal nerve roots: that was a famous controversy. Alfred Russell Wallace and Charles Darwin on the principle of natural selection: that was a famous gentlemen's agreement about the independence of their separate similar thinkings. Adams and Leverrier, each calculating the existence of the planet Neptune in 1845: it was a natural calculation to make just then. Gray and Bell each inventing the telephone in 1876 and fighting afterward over the patent rights. James in 1884 and Lange in 1887, each with the same physiological theory of emotion. Mach in 1886 and Avenarius in 1888, each with what they agreed was the same epistemological theory of the relation of mind to matter. Nearly all the others of Ogburn and Thomas' illustrations (1922) can go in here. You cannot get away from the fact that the times have something to do with discovery, something more than the fact that discovery must wait upon the necessarily antecedent discoveries.

5. *Apparently independent discoveries may nevertheless depend on some degree of unconscious communication.* The human mind being as it is, these cases must occur, but it is hard to prove them. Was Newton influenced in the calculus by Isaac Barrow? Why did Titchener, when he formulated the context theory of meaning in 1909, feel that he was making so original a contribution when the basic relation had been noted by Bishop Berkeley in 1709? Titchener knew his British associationists and he was being just exactly two centuries late; yet he never seemed to realize this fact.

Unconscious plagiarism also belongs in this class. Was Alexander Walker one such? When Bell and Magendie were disputing priorities about the law of the spinal nerve roots, Walker claimed priority over both, saying that he had lectured on the law in 1809, two years before Bell published, thirteen before Magendie. It happens that Walker got the specification of the spinal roots reversed and his claim is now treated only with amusement, but what was going on in his mind? Had he heard Bell lecture? Could he have been faking or was this a case of unconscious as well as faulty assimilation?

6. *Prestige, authority and good promotion may distort the historical record so that of two discoverers the more famous or more aggressive man gets the greater credit.* Prestige is autocatalytic. Greatness tends to build itself up. Fame is outfitted with positive feed-back. But promotion helps. Magendie was a better promoter than Bell and for a while he eclipsed Bell in their argument. Johannes Müller was a better promoter than Bell, and the doctrine of specific nerve energies was known as Müller's until long after the death of both men, in fact until a champion for Bell belatedly appeared. After all, the doctrine with that name was Müller's, for Bell gave it no name and did not elaborate it as a doctrine. He thought the principle was obvious. It was Müller who built the obvious up into a pronouncement. Broca had an aphasic patient die on his hands in 1862. At once he examined the brain, found deterioration in the left frontal convolutions, reported the matter and presented the brain in alcohol to the Société

d'Anthropologie. So dramatically was the announcement made that Broca's speech center was turned into a scientific fact. Broca's anticipators, Bouillaud and Dax, never had a chance to get their names attached to this fact. Bouillaud, as a matter of fact, was out of phase with the Zeitgeist, for he made his discovery in 1825, when exact localization in the brain had been made unpopular by Gall's phrenology and by Flourens' excellent experiments which showed common functioning of large regions of the brain.

We can go on. The new experimental psychology was founded by Wilhelm Wundt with all the arts of founding a science—the writing of the new epistemological formula, the publication of the authoritative handbook of the new science and the keeping of it up-to-date through six editions through twenty-seven years, the establishment of the world's first formal research laboratory, the publishing of the world's first journal of experimental psychology, propaganda, controversy, argument, research, Ph.D. students scattered abroad in Europe and America—there you have the technique of founding a science. But Wundt did not create the science. Fechner and Helmholtz had as much claim to that, and astute observers say that the new psychology was born of the Zeitgeist. Wundt was a vigorous agent of what the times were about, a prophet, as it were; but prophets expound the truth, they do not make it.

7. *Exceptional insight may lead to an original discovery which has not been anticipated by others and which is relatively independent of the times.* Presumably no discovery is ever entirely independent of the times but there are degrees of independence and originality. Newton's theory of color was one such instance, his discovery that white light is a mixture of colored lights. We feel that that discovery was not all ready to be made by someone else if Newton had failed, because Newton's senior colleagues in the Royal Society, when they read Newton's letter on the subject in 1672, ridiculed the absurdity that white can be a mixture of colors. Yet suppose that Newton had reacted to the ridicule, had abandoned his plan to write an *Opticks* to remain satisfied with the *Principia*. Do we think that no one would have found out about white light's being a mixture of homogeneous lights? That seems quite impossible. The discovery could in that case have waited a century, but late in the eighteenth century the times would surely have created some necessity for this discovery and it would have been made. Would the Zeitgeist have waited for Thomas Young, or is that question prompted by the faith that personal genius is half the reason for discovery and the Zeitgeist only the other half?

Perhaps the best example of insight is Fermat, an excellent example because the insights of his genius have not fully been equalled since. The story about Fermat is that he noted on the margins of his copy of Diophantus his comments about solutions of certain theorems. Not always did he give the solution but mathematicians since 1637 have been able to prove all his assertions right except one. That is Fermat's "last theorem," that $x^n + y^n = a^n$ can be true for whole numbers only when n is not greater than 2, as in $3^2 + 4^2 = 5^2$. Fermat wrote in the margin that he knew a splendid proof but had no space there to write it down, and mathematicians believe him because he has been proved right in every other case. In that insight of Fermat's there is something which was not an inevitable product of the Zeitgeist, because it did not happen again and the passage of the centuries did not force a discoverer to come on the stage of science and perform his act. The naturalistic theory has its place for Great Men, for not all brains are alike.

8. *An accident may lead to a discovery.* This is Conant's case of the "happy accident." Presumably the accident may play the same role as insight, and, as we shall see presently, insight works by trial and error. There may be no case so dramatic as Fermat's, a case where an accident led to an original discovery which might otherwise have remained unknown for centuries, but there are lesser instances. Galvani discovered the electrical properties of frogs' legs because, when the legs were set up in certain ways, thunder storms made them twitch. Oersted is said to have discovered the magnetic field of the electric current during a lecture demonstration. Becquerel in 1896 left a uranium compound on a covered photographic plate and found later that it had affected the plate. That was the discovery of radioactivity. There are many other instances. A new fact is a new relationship, and these relationships can first be formed in the mind by insight or in nature by chance. In the latter case there has to be an insightful mind to note the relationship and to make use of its significance.

9. *A discovery or an advance in knowledge may be prevented from acceptance or delayed in acceptance when it runs counter to conventional thought.* This is the inertia of the Zeitgeist, the way in which it slows or prevents progress. Al-

most never, says Conant, will contradictory facts drive out an old theory. It takes a new theory to displace an old. That is because convention supports the old theory in the face of contradiction. When Kepler correctly described the physics of the eye, noting that the crystalline body must be a lens and that it would form an inverted image of the external world on the sensitive retina at the back of the eye, he was puzzled as to how we see right-side up when the retinal image is upside down. He was still thinking of the mind as a perceiving little man within the head, one who looks at the images and impressions which the nerves bring to it. Kepler wrote in 1604. By 1691 Molyneux had the right answer, that *up* and *down* are relative and that the brain does not look at the retina but perceives only the relationship. Berkeley had the right answer in 1709. Johannes Müller had it in 1826, Helmholtz in 1866. Each of these men had to solve the problem over again and try to convince his audience for no better reason than that conventional theory about the mind has always been personalistic and that only with difficulty can a man come to think of his perceptions as neural responses and not as contact with an actual object. For centuries now the Zeitgeist has favored the personalistic conception of mind.

Let that end our macroscopic survey of the ways in which the discovery by one man is dependent upon the discoveries and thought of others. Now let us enquire what goes on at the microscopic level, in the intimacy of the consciousness of the Great Man.

ATTRIBUTES OF GREATNESS

It becomes plain that we have now rescued the Great Man from the toils of the Zeitgeist without abandoning naturalism. The Great Man is a natural object, a brain, if you please. And the events in that brain are natural events, which depend upon the properties of the brain and the conditions, physical and social, which instigate its particular actions. Not all brains are alike. Apes have brains but no good science. Nor are all human brains alike. There are Great Men, but they operate with different degrees of native ability under the limitations and the facilitations of the situations in which they find themselves. The Zeitgeist at any time facilitates such mental activities as are consistent with its conventional habits and attitudes, and it hinders what is contrary to conventional thought or is otherwise out of phase with the times.

In a sense it is true that originality in the Great Man varies with the degree to which his thought is independent of the Zeitgeist, but such originality is only part of what makes the Great Man great. If he is too far out of phase with the Zeitgeist, the world may not listen to his message and he may die in obscurity. Perhaps he can overcome the resistance of the Zeitgeist by well chosen promotion which will gain acceptance for his revolutionary ideas, but then he needs, not only originality, but also energy, aggression, practical wisdom, and the will to compete. Or else he needs a sponsor, who may indeed be a posthumous sponsor, to promote his theories and point out their significance. So originality may merit Fame, and Fame go actually only to Recognized Originality. Galton ran into that difficulty in 1869 when he sought to use Reputation as a measure of Genius. Originality may measure genius, but the Great Man requires recognition in order to be great.

It thus appears that there are certain extrinsic attributes of greatness and we may mention three.

(1) The great discovery cannot occur until the necessary *antecedent discoveries* have been made. Discovery is serial and no amount of originality will enable a man to base an advance in knowledge on previous ignorance when what he needs is previous knowledge.

(2) Being *in phase with the Zeitgeist* helps the attainment of greatness, even if it lessens the degree of genius required. Presumably the Zeitgeist effectively prevents some discoveries, some revolutionary thoughts, but it must be regarded more as an accelerator and a brake than as furnishing necessary or sufficient conditions for discovery.

(3) And then varying degrees of *promotion* are required in order to gain acceptance for new facts or attention to them. If the promotion is posthumous—like De Vries' promotion of Mendel's discoveries—the greatness may also be posthumous. When greatness occurs *in vivo,* unpromoted by either the Zeitgeist or an enthusiastic colleague, then it is apt to be due to self-promotion, and we find ourselves adding *aggressiveness* to the list of the attributes of the Great Man. It is not necessary to cite here the names of scientists who have believed in their own correctness and who have said what they believed about themselves. We have already noted how Wundt brought all the means of promotion to bear on the establishment of the new experimental psychology. His vigor in controversy, his unrepressed

conviction of his own correctness, were not, in spite of their force, self-validating.

When we turn our attention to the more important intrinsic attributes of greatness, it becomes obvious that the specifications of genius must be made in respect of *insight* and *motivation*. We need first, however, to consider the nature of insight and how it enters into original thinking.

INSIGHT

The ape, unable to reach the banana through the bars of his cage with either of the short sticks in his possession, finds by accident that the two sticks fit together like a fishing rod to make one long stick, pauses, looks toward the banana, back at the newly acquired long stick, rushes over and rakes the banana in. That is *insight*. Insight is the perception of a novel relationship. It can occur in thought with respect to absent objects, and then visualization is useful but not always essential. Let us examine three instances.

In figure 1 we have two separate pictures, *A* and *B*, each mounted on cardboard. The problem is to arrange these two pieces with respect to each other so that the two riders will be astride the two horses. Trial generally leads to errors. The riders are upside down and they do not fit in properly. The solution generally comes by chance manipulation or by the solver's being shown what to do, but once he sees the trick, once insight occurs, he has learned the problem permanently.

The problem is solved by turning the strip *B* through 90 degrees with respect to picture *A* so as to give the solution shown in figure 2. In figure 2 the forelegs of one horse in figure 1 are combined with the hind legs of the other horse. It is easy to see how predisposition prevents the solution of this simple problem. When you wish to have a horse, you do not pick up the essential parts from two different animals—not often.

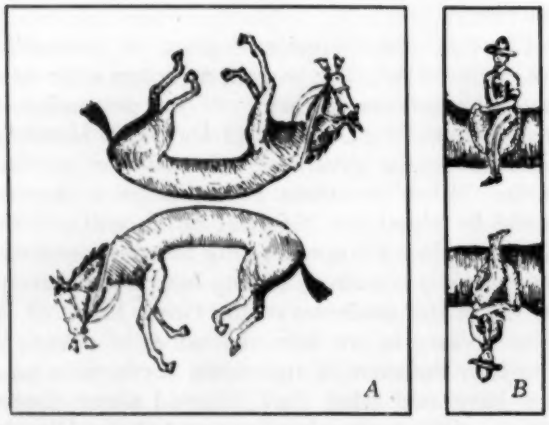

FIG. 1. Horse-and-Rider Puzzle. Problem: to place the riders astride the horses right-side up. From M. Scheerer, K. Goldstein and E. G. Boring, *Amer. Jour. Psychol.* **54**: 439f., 1941.

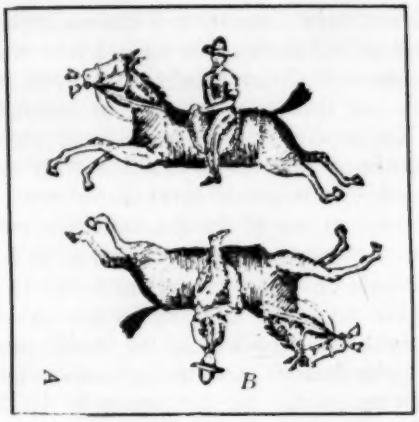

FIG. 2. Horse-and-Rider Puzzle. Solution of problem in figure 1.

The horse and rider problem is usually solved by the trial and error of manipulation, but the solution of the ring problem is usually a case of ideational trial and error. You work by vicarious trial and error (in Tolman's phrase), by hypothesis and the thought experiment. Figure 3 shows the device for the ring problem. The block *P* will not pass through either of the rings, *R* and *S*, nor will either ring pass through the hole *Q*. The problem is to get both rings on one loop. Able minds have puzzled for hours over this solution.

The correct solution is rational. The solver sees that to get the ring *S* on the loop *ABC*, *S* must be passed along the cord to *H*, through the hole *Q* at *G*, around the loop *FE*, back through the hole to *D* and *C*, a series of events which puts both *S* and *R* on the loop *ABC*. This reasoning is correct except for the difficulty that the ring *S* will not pass through the hole *Q*. At that point the solver is frustrated, but there can come to him the needed insight. Usually it comes suddenly.

The solver sees that the ring is to pass through the hole only in order to be passed around the loop on the other side of the hole and then to be brought back to the side where it was at first. The ring will not, however, go through the hole. Nevertheless, since the ring would go through the hole only in order to go around the loop and then to come back again, there is the possibility—this is the moment of insight—of pulling the loop through the hole, letting the ring go around it, and then pulling

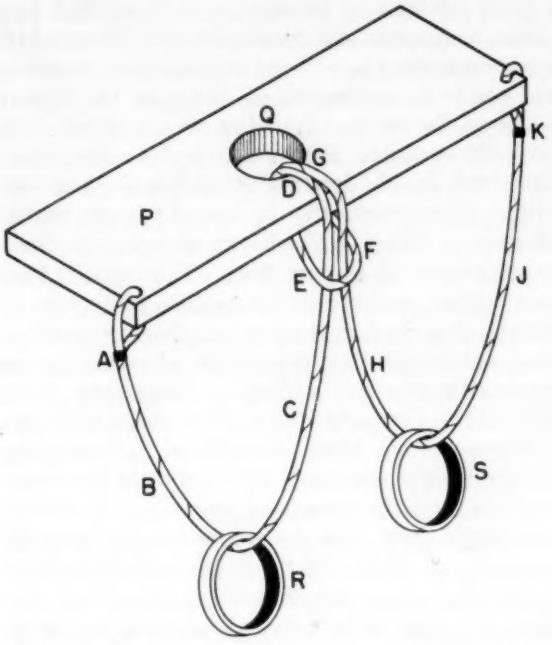

FIG. 3. Ring Puzzle. Problem: to place both rings together on the same loop. After Max Wertheimer, *ca.* 1941, presumably unpublished.

the loop back where it belongs. That works. The cord may seem to get tangled in the process, but eventually everything straightens out and the rings are found side by side on the same loop.

This insight comes near to the sort of creative process which the able scientist uses. The insight in figures 1 and 2 resembles, on the other hand, discovery by "happy accident."

As a rule the investigator does not make his discovery with a single insight, but with a series of hypotheses which are trial-insights and which may, any one of them, be seen to be in error, sometimes as soon as it is formed, sometimes only after its consequences have been investigated. The process of discovery thus comes to resemble the process of trial-and-error by which the rat discovers his food in the maze, except that there is in science more vicarious trial-and-error (thought experimenting) and less physical exploration of the blind alleys. The rat starts out at random or follows a hunch, tries an alley, abandons it when he finds it blind, tries another, keeps on by trial-and-error until he finds food. The investigator forms an hypothesis, a tentative insight, tries it out in thought or experiment, abandons it when he perceives it wrong, goes on to another if the first has produced progress, and finally reaches some success which is his goal or else surrenders frustrated. The analogy to this process we get in the block problem.

Figure 4 shows five blocks, *A, B, C, D* and *E,* piled on top of one another, always a smaller on top of a larger, and it shows three possible places where the blocks may be placed, I, II, and III. The blocks must be moved one at a time, placed only in one of these three positions, and never a larger block on top of a smaller. The pile of blocks is initially in position I, and the problem is to move them to position III in the minimal number of moves, and ultimately to determine the law of least action. Here is the way the insights generally go when they lead to success.

(*a*) The solver decides first to learn how to work with fewer than five blocks. If he uses only one block *A,* insight into the solution is instant. You move *A* from I to III. It takes one move. The problem is solved.

(*b*) With two blocks, *A* and *B,* it takes three moves. Insight shows at once what to do. You move *A* from I to II, *B* from I to III, then *A* from II and III on top of *B.*

(*c*) With three and four blocks immediate insight usually breaks down, but the solver gets by induction from his trials, errors and success this rule: If *n,* the number of blocks is odd, the top block must be moved first to the goal position (III), whereas, if *n* is even, then the top block must be moved first to the other position (II). This is a correct but unsatisfactory hypothesis. It is not clear why it is true.

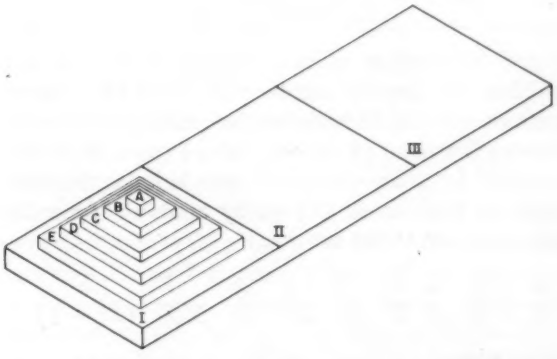

FIG. 4. Block Problem. To move the pile of blocks from I to III, one block at a time, without placing a larger block on top of a smaller or placing any block in a position other than I, II or III; and to make the number of moves a minimum; and to determine the rule of moving and the formula for the minimal number of moves with any number of blocks. After J. C. Peterson, cited by W. S. Hunter, *Human Behavior,* 331f., 1928.

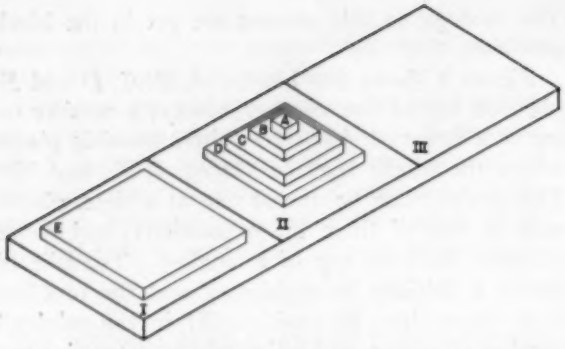

FIG. 5. Block Problem. Partial solution. The problem has been solved for four blocks A to D which have been moved from I to II. It is still necessary to move the block E from I to III, and then to repeat the four-block solution in moving the blocks A to D from II to III on top of E. Thus N for 5 blocks = (N for 4 blocks) +1+ (N for 4 blocks), and $N_n = 2\ N_{n-1} + 1$.

(d) The next insight consists in seeing the relation of the problem for n blocks to the problem for n–1 blocks. Figure 5 shows a stage of the solution of the problem of figure 4. To move the five blocks from I to III, as required in figure 4, you must first move the top four blocks from I to II. Then you can move E to III, and then the four blocks from II to III. If N_n is the minimal moves for n blocks, then N_5 is N_4, plus the move of E from I to III, plus another N_4; which is to say,

$$N_5 = N_4 + 1 + N_4 = 2N_4 + 1$$

or, more generally,

$$N_n = 2N_{n-1} + 1 \quad \ldots\ldots\ldots (1)$$

(e) The solver is now equipped to solve the problem for any n, but he still lacks the general formula for N_n. If he is mathematically minded he derives it from (1) above. More often he writes down N for many small n's and induces the rule. Thus he finds from (1) or by applying the rules and counting actual moves:

n =	1	2	3	4	5	6	7	n
N =	1	3	7	15	31	63	127	$(2^n - 1)$

which gives

$$N_n = 2^n - 1 \quad \ldots\ldots\ldots\ldots (2)$$

The problem has been solved by a series of insights. It was not solved by manipulative trial and error.

The history of science contains many instances of crucial insights, each of which brought about a great advance in knowledge and was had by a Great Man, often at a specified time. Many of the instances appear to belong to the folklore of science and not to be authenticated, but even the legends illustrate the point. Whether he did or not, it is perfectly possible that Archimedes could have conceived the hydrostatic principle in his bath with such assurance that he leaped out and rushed off crying, "Eureka!" without stopping to make an experimental test or two. That would have been easier under the rationalistic Zeitgeist of Greece than under modern empiricism, but we must not forget that theoretical physicists today accomplish wonders without a laboratory (Bell, 1937: 29). Descartes' second dream on the night of November 10, 1610, is supposed to have given him the insight into applying algebra to geometry, and thus the basic conception of analytic geometry —although just what form the insight took we do not know (Bell, 39f.). It is undoubtedly true that Galileo's first insight into the law of the pendulum occurred as he watched the swinging lamps in the Cathedral at Pisa, noting that the time of the swing depended more on the length of the suspension than on the amplitude of the swing (Wolf, 1935: 43f.). Voltaire's story of Newton and the apple is held to be apocryphal, but there must have been some moment when Newton first saw that the moon could be held in its orbit because its constant fall toward the earth was balanced off against its constant centrifugal departure from the earth (Bell, 105f.; Wolf, 149f.). William Hamilton's prediction of the principle of conical refraction was not the result of an instant of insight but the consequence of mathematical reasoning, as was Clerk Maxwell's prediction of the nature of electromagnetic waves. These well authenticated instances represent theoretical physics at its best. The empiricists verified the predictions later, but the genius lay in the prediction (Bell, 350f.). Comparable to Descartes' dream and better specified was Fechner's reverie in bed on the morning of October 22, 1850, when he thought he saw how to resolve the conflict between materialism and spiritualism by measuring the magnitude of an immaterial sensation in terms of a material stimulus. From that insight was born psychophysics and a large portion of early experimental psychology (Fechner, 1860: 2: 553f.). A recent instance is related about an insight of Max Wertheimer, an idea which began the whole movement of Gestalt psychology. Wertheimer was in the train going from Vienna to the Rhineland in the summer of 1910 when an idea came to

him with such force that he got off the train at Frankfurt, bought a toy stroboscope, and went to his hotel room to experiment on seen movement. He stayed on at Frankfurt for two years, completing those now famous experiments on perceived movement and the phi-phenomenon which generated a whole new school of German experimental psychology (Newman, 1944: 431f.).

While it would be interesting to know just what happens in the mind of the Great Man when he has the crucial insight, it is not certain that precise information would greatly increase our knowledge of the attributes of greatness. Some of these insights are legendary and have been dramatized in the telling and retelling. It is also true that a man who has had an insight seldom knows just what occurred and may be so moved by enthusiasm for his sudden revelation that his descriptive objectivity is impaired. Descartes' dream and Fechner's early morning reverie were both great emotional experiences to their possessors, were indeed revelations of the order of conversions, and both these great men may have attached more importance to the moment of alleged insight than the facts, if they could but be known, would warrant. Enthusiasm, as we have already remarked, is not self-validating. Take the simple controlled insight of seeing how the ring puzzle (fig. 3) is solved. Who is there, that has had that insight, and can say *how* it occurred? At one moment you are frustrated. Then, if you are a visualizer, there is a breathless instant in which you see the loop being pulled through the hole and you wonder if that might not carry the solution, and then, suddenly, you see that it *is* the solution, that the ring can pass around the loop if the loop is brought through the hole to the ring. But to know all that is not to say *how* insight occurs, much less to say *why*. There is still an adventitious leap from nothing to something, an unconscious leap whose continuity and causes remain hidden in the nervous system.

ATTRIBUTES OF GENIUS

We have mentioned (1) previous knowledge, (2) the Zeitgeist, and (3) promotion as extrinsic factors contributing to greatness. We may now continue the list with the intrinsic attributes of greatness and thus also of genius.

(4) Certainly *erudition* is a property of those brains which have many correct and successful insights. Any bit of information may turn out to be unexpectedly relevant and nothing short of erudition provides enough material for the unpredictable requirements of genius. When Clerk Maxwell was explaining how Faraday got the electric current to flow continuously through a circuit when he moved a wire of the circuit through the magnetic field, how Faraday managed that effect by reversing the terminals of the outside circuit whenever he had to reverse the movement of the wire to shift it back through the field, Maxwell remarked that the invention was like Cyrus' when he had wished to get his army across the Euphrates. Cyrus sent his engineers upstream to alter the course of the Euphrates so that it would flow behind the army. If only Faraday, the discoverer, had had that insight instead of Maxwell, the biographer, we would have seen how an advance in physics can depend on historical knowledge.

Expertness is limited erudition, and often one kind of expertness will transfer to a problem in another field. The man who knows about rope tricks is all ready to solve the ring puzzle (fig. 3). The boy who saw at once that 39 times 41 is 1599 had the insight that 39×41 is $(40 - 1)\ (40 + 1)$, which is $40^2 - 1^2 = 1600 - 1$; but he had to know algebra or he could not have had the insight. He could also have realized, with the same insight, that $(10m + n)\ (10m - n)$ is $100m^2 - n^2$, that $94 \times 86 = 8100 - 16 = 8084$.

It is said that a genius needs a *good memory* and there are, of course, individual differences in memory. Erudition, however, must include good memory. He who learns and forgets is not erudite.

(5) There is no good word with which to label interest in novel relations. We might perhaps call it *xenophilia*, love of the strange, the unexpected, the unconventional. Named or unnamed, that set or attitude must constitute a variable of the personality. The genius is constantly on the alert for what is new. The conventional irks him; the novel attracts him. He is the supreme punster because he is always bringing the previously unrelated together and trying out the effect of combining them. Treating geometry with algebra was an Olympic pun, a mating of the previously unrelated to create, actually, analytical geometry, that indispensable servant of science and mathematics. Perhaps this attitude of being receptive to oddly novel relationships can be learned in youth or perhaps it has to be established adventitiously in infancy. We do not know. Certainly some persons delight in bringing the unusual together and others do not. Certainly the insights

of genius, as of the puzzle solver, are favored by the xenophilic attitude.

It has been said that *availability of memory traces* is an attribute of genius, but that trait is either good memory or a large repertoire of knowledge or this special receptivity to novel relations. It is scarcely something new.

(6) *Visualization* is a special instrument of thought. People vary in respect of it and the capacity can be improved by training. It is useful in detailed memory, in insight into spatial or mechanical relationships, and thus into all logical relationships that can be represented by spatial design. It is not a *sine qua non* of many intellectual operations, but it is a natural tool, and the genius, who cannot visualize when he needs to, must have some other qualities to make up for his lack.

There are also the *motivational factors* which enter into genius.

(7) *Alertness* is one. Genius may not be an "infinite capacity for taking pains," but neither is it lazy nor lethargic. Nor is it reverberatory. It moves on restlessly to new situations.

(8) Nor does genius—or so it seems to me, for there is no scientific evidence for this point—gather wool. Most consciousness—as the writers about the stream-of-consciousness have shown—is full of wool gathering, the silly eddies of thought which retard the main current, the futile sallies up a blind alley once and then back and up the same alley once again. It is not true that the genius thinks faster per second than the ordinary man, or even per minute. Nervous impulses work for everybody at the same rate. But the genius seems to do more effective thinking per year and often per hour. How does he manage that? By good steering. By sticking to the goal. By not stopping to rest. By avoiding the irrelevant when it becomes known for what it is. By perceiving mistakes clearly and then not repeating them. In other words, inefficient thinking may proceed at the same rate as efficient thinking and yet take longer to reach the same goal. When Sherlock Holmes had observed that the killer could not have left the room, he did not waste time in doubting his own sound observations. He accepted his own conclusion that the killer was still in the room, improbable as that seemed, and sure enough, with the help of Conan Doyle, Holmes was right, for he was a genius.

Do we need a name for this steady directedness of purpose which keeps the genius still on the track while forging ahead? It is more than persistence. It consists in entertaining and assessing the novel, forging ahead, but keeping one purpose to the fore. Would *incidence* do for a name? This kind of thinking aims straight, though it can be deflected with reason.

Certainly this list of attributes must be far from complete. The question of what makes thought creative has had considerable attention, yet not nearly enough. We are now in the United States "training," as we say, thousands of young scientists. We teach them facts. We practice them in the use of methods. We endeavor, in ways not too clearly understood, to inculcate certain attitudes in them. And then, having done our best, we wonder why so seldom the spark of originality is found—for we never expect to uncover the flame of genius. Perhaps this *plus*, which is neither prevented nor assured by training, is not got by training at all but can only be captured by selection on its adventitious occurrence. Or perhaps, on the other hand, it could be created in young men if only its nature were better understood. We need to know better than we do now what makes Great Men great. It is not all the Zeitgeist. Some brains are better than others. Why? Does the difference lie in training? Do we know how to make the better brains or where to find them? Perpetually we tinker with curricula for making poor brains good, and yet we do not know just what it is that we should like to achieve, what the attributes of much-wanted Genius are.

Whatever the final description, I suggest that *insight* and *motivation* are satisfactory class names for two sets of parameters which are the dimensions along which civilization will be enlarged or lost. Carlyle was partly right. The History of Civilization is the history of the operation of Genius. We have, have had, and must have Great Men, even though no one of them could have arrived without the benefit of his Zeitgeist.

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THE WENTWORTH PAPERS OF BURKE, ROCKINGHAM, AND FITZWILLIAM[1]

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PROFESSOR COPELAND, in the introduction to his recently published essays on *Our Eminent Friend Edmund Burke*, ventures the prediction that "we are entering upon a period in which Burke will be more actively studied than he has been in at least a century." The basis of this prediction is not the revived relevance of Burke's political wisdom to the problems of living men—although I am sure Professor Copeland would recommend the study of Burke for that important reason—but because an enormous mass of precious historical materials (hitherto little studied, or not studied at all) has this year become accessible to scholars. They are the private papers of Burke and his principal political friends.

This interesting event appears as one of the happier results of the "revolution" that is converting a good many of the "stately homes of England" into edifices for housing public institutions. One of the greatest, Wentworth Woodhouse—a Georgian palace built by the second Marquess of Rockingham—has been leased by the authorities of West Riding for a teachers' training school; and only a twenty-room flat, I was told, is reserved for use by its owner, Earl Fitzwilliam. The alterations required a clearing of muniment rooms and removal of a vast store of documents, some of them as old as seven hundred years. Much of this material has been deposited in the Sheffield Public Library on a fifty-year lease, and there it is being arranged for library record, although title of ownership remains with Lord Fitzwilliam. All of it may be studied, but none of the documents may be reproduced without the permission of the librarian or of Earl Fitzwilliam and the trustees of his estates.

The Burke, Rockingham, and Fitzwilliam papers are only a fragment of the Wentworth mass, but even so their dimensions are large. During two months of steady application from ten in the morning to six in the evening I was able to read only a small fraction of them, and even with my wife's help I could make no more than a rough inventory-survey of the whole—or rather of what I thought was the whole. Perhaps the lorry had not ceased running from Wentworth to Sheffield, when we sailed for home, or it may have been that the library staff who unpacked the boxes in the cellar had not finished their job; but the fact is that Professor Copeland, who arrived in Sheffield shortly after I did and stayed after I left, toward the end of the summer wrote to tell me that about five hundred additional letters *to* Burke had come to light. And I thought I had seen everything!

Partly for that reason, but chiefly because of the limited time now at my disposal, I can do no more than hint at the wealth of this collection. It is really *one* enormous documentation of the history of the Whigs who followed Rockingham's leadership and then, after his death in 1782, constituted the party of the old Whigs who separated from Fox in the 1790's. The three collections interpenetrate and when the Historical Manuscripts Commission calendars them they probably will be arranged in one series under the name Wentworth Papers. Such was the tentative idea of Professor G. R. Potter, of Sheffield University, who is to direct that work for the Commission—a task that will not be completed for many years.

Of the Burke papers, properly so-called, I shall say little, lest I trespass in Professor Copeland's territory. We found them folded and packed in boxes and bundles, probably as they were left by those privileged characters, Dixon Wecter, Sir Philip Magnus, and the late Canon Robert Murray, who examined them a decade or more ago. Magnus used a portion of this material in his biography of Burke. Murray made transcriptions of some of the letters, and we are all familiar with the admirable studies that grew out of Professor Wecter's researches at Wentworth. Professor Copeland and I kept at hand his main one—*Edmund Burke and His Kinsmen*—as we examined these documents; and we could assure you—if any such assurance were needed—that Professor Wecter was a thorough and meticulous sifter. There

[1] Paper presented at the meeting of the American Historical Association in Boston, December 30, 1949.

are perhaps 2,500 letters. Most of them are from Burke's correspondents but there are perhaps as many unpublished ones of his own as appeared a century ago in the four volumes edited by the third Earl Fitzwilliam and Sir Richard Bourke. The original manuscripts of these latter also were packed in the Wentworth boxes. The rest of the contents were notes for speeches (very hard to identify or decipher) some unfinished drafts of half-started writings not published by Burke's literary executors, and various odds and ends that might be described as the sweepings of an untidy man's desk. In all this I doubt there is much important information about Burke's relations with his kinsmen, Richard and Will, or about his and their financial affairs, that escaped Professor Wecter's eye. But I am sure that neither he nor Sir Philip Magnus would pretend to have exhausted the Burke papers.

It is my opinion that there is much material here for fresh and worth while study of the following phases of Burke's biography and the political history of his times: the repeal of the Stamp Act, British commercial policy, the North American trade act of 1766, *Nullum Tempus* in 1768, Irish politics from 1761 to 1766 and from 1773 to 1782, Catholic Emancipation in England and Ireland, war and peace questions from 1778 to 1782, economical reform, the Fox-North coalition, the trial of Admiral Keppel, the politics of the Regency question in 1788–1789, the deep roots of the quarrel with Fox, the Burke-Chatham antagonism, Burke's relations with his son Richard and Lord Fitzwilliam, East India Company affairs especially in 1766–1767 and 1772–1773. On all these subjects much remains to be said and much is to be learned from the study of these papers; which study of course should be extended to the additional Burke manuscripts that were long deposited at the Milton house of the Fitzwilliam family but are now available for scholars at the Northampton Record Society at Lamport Hall, Northamptonshire. Sir Philip Magnus told me the Milton papers are about one fourth as many as those from Wentworth. I have not seen them, but Professor Copeland has and perhaps will speak to you of them.

Of great importance, I think, are the hundred-odd Wentworth letters that were written to Edmund and William Burke by one Charles O'Hara in Ireland from 1761 to 1776. Until recently the very fact that there was such a correspondence does not seem to have been known to Burke's biographers. Sir Philip Magnus expressed surprise when I spoke of them because he had no memory of having seen them. I do not know whether they drew Professor Wecter's attention. O'Hara's script is very hard to decipher and I found them slow-going not only for that reason but because many are undated (as to year) and filled with minute political information, Irish, local, and obscure. But Charles O'Hara can easily be identified as a landed gentleman at Annaghmore, a high sheriff of Sligo County and member of the Irish House of Commons, a son-in-law of the Earl of Hyndford, and brother-in-law of an Archbishop of Dublin; and it was plain from the letters that he was an intimate family friend of the Burkes. While I was wondering if the other end of this correspondence might still exist, Professor Copeland turned up to tell me that Canon Murray in 1947 had obtained transcriptions of certain letters from Burke to one Charles O'Hara, and that these transcriptions were among Murray's literary remains in the Bodleian. I then got in touch with the Director of the National Library in Dublin who is energetically ransacking Ireland for old historical records, and he soon replied that he had found 125 letters from Edmund and William Burke in the hands of a family named O'Hara at Annaghmore. He had them brought to Dublin and microfilmed and kindly provided me with a reel of the film. On it my seminar students are now at work. The Burkes and O'Haras can thus be reunited. These letters are, I suggest, the most important new source of knowledge about Burke and his kinsmen since Sir James Prior's fifth edition of the *Life* a century ago.

I say this because the Burkes wrote to O'Hara in terms of almost family intimacy. Many of these letters were dashed off in great haste with little attention to the rules and conventions of writing. The Burkes were eager to tell O'Hara of everything important in their lives whether political or personal. We all know how reticent Edmund Burke was about his private affairs; here that reticence hardly shows. I will give you a sample. On January 18, 1766 Burke wrote to tell his friend of his first speech in the House of Commons which he had made the previous night. It is the only account from Burke himself that we have of this occasion:

> That day I took my first trial. Sir William Meredith desired me to present the Manchester petition; I know not what struck me, but I took a sudden resolution to say something about it; though I had got it but that moment & had scarcely time to read it, short as it was. I did say something; what it was I know not

upon my honour. I felt like a man drunk. Lord Frederick Campbell made me some answer to which I replied; ill enough too; but I was by this time got pretty well on my legs. Mr. Grenville answered. I was now heated & could have been much better; but Sir George Savile caught the speaker's eye before me & it was then thought better not to proceed further. . . . However, I had now grown a little stouter, though still giddy and affected with a swimming in my head; so that I ventured up again on the motion & spoke some minutes, poorly, but not quite to so ill as before. All I hoped was to plunge in and get off the first horrors; I had no hopes of making a figure. I find my voice not strong enough to fill the house; but I shall endeavour to raise it as high as it will bear. This is probably like a child to a father. Whenever I enter into these minutiae about myself I beg you throw the letter into the fire.

Answering this letter a week later, O'Hara said:

Give me leave to know you, tho' I know nothing else so well, better than you know yourself; your voice will form from practice, your manner will improve. The great point you are to attend to is temper. Was it not Jephson that used to tell you that in some circumstances you had an air of anger? Get rid of that air.

There are so many nuggets of pure gold in this mine that I wish to present one more. It is from Will Burke and the date is October 4, 1766.

If Ned gets to you . . . he will be able to tell you that our fortunes are in a condition to second our views of independency, and our resolution of acting in our public capacity with the same correctness as we have had the good fortune to observe in private life. You will be glad to know that in this we have no division of our obligations; all this, like as the all before, we owe to Lord Verney's wonderful goodness and friendship. In one word, the necessary rise of value of East India stock was foreseen, before the price rose or our increased dividend was talked of; but as that increase might possibly not be determined on in 3, or 6, or 9, or even 12 months, those who bought on what they call speculation, that is, who agreed to pay such a price for such a quantity at a particular day, ran the risk of loosing if the price at that particular day happened not to answer his speculation, so that no one could venture on buying with safety but those who could actually pay down their money, and keep their stock in their possession quietly till the dividend was increased. This Lord Verney could, you know, easily do and he had chosen to lay out a million that way. No one could have objected to his taking the consequential benefit of all the money he employed that way, but he considered this as an opportunity of making us independent, and actually paid down of his own above 9000 £ and engaged for above forty more for me. The dividend is come sooner than I expected, and tho the accounts are not yet settled, I may within compass say that I have made 12000 £ at least. It would be idle to use words to express what we owe to this man's disinterested, unaffected worth and goodness to us. . . . Surely we may think it providential, and without any superstitious vanity too, if the thought of it reminds us to endeavour to grow better men as we grow richer. It is our good fortune you see to have this advantage without even the imputation of stock jobbing, or the term bull and bear being applicable to us.

I submit that this is a contribution to solving the much-discussed mystery of the Burke finances!

I turn now to the Rockingham papers, but the first item to notice briefly keeps us on the subject of Burke. One day Professor Potter was rummaging in the basement of the Sheffield Library and found among the Rockingham boxes a folio-sized notebook which he kindly brought upstairs and handed to me. On the cover was written NEW YORK, and it proved to be the book in which Burke preserved the dictated drafts of his letters to the Committee of Correspondence of the New York Assembly during the years 1771–1775 when he served as that Assembly's agent in London. Since but one complete letter and parts of two others were all that hitherto had been known to have survived of this correspondence, it was evident that an important historical discovery had been made. Here were the materials for reconstructing one of the most obscure phases of Burke's political biography. When they are published I am sure it will be agreed that some important new light has been thrown not only on Burke's politics, but on the colonial history of New York and the genesis of the American Revolution. I asked and obtained from Earl Fitzwilliam permission to publish these letters, and after I left England the New York end of the correspondence turned up in those additional letters to Burke which Professor Copeland fortunately was on hand to examine. He ordered a microfilm of them, so that I shall be able to join the ends and present a very complete documentation of the history of Burke and the New York Agency.[2]

An interesting fact that comes to light in this letter-book is that, although Burke's election as Agent was first proposed by the more radical Whigs in the Assembly, the member to whom he wrote confidentially was James DeLancey, head of

[2] The American Philosophical Society, of Philadelphia, has kindly given a grant-in-aid to assist the preparation of this study.

the dominant conservative party. DeLancey was a friend of the Marquess of Rockingham and the leading New York loyalist in 1775. Burke may have owed his appointment far less to the Livingston-Schuyler Whigs than to the influence of Rockingham exerted through DeLancey. That is only tentative speculation, but it is plain from the letters that Burke did his best to cultivate the imperial loyalty of New York, that he cared much more about preserving the British Empire than redressing colonial grievances, that he detested colonial radicalism no less than the homegrown species, and that he believed American hotheads were as much to blame for the Anglo-American quarrel as the misguided ministers of George III. How closely he kept in touch with other American agents may be suggested by the fact that in all these letters the name of Benjamin Franklin was never mentioned!

But to return to the Rockingham papers proper —I found among them at least nine hundred letters of the Marquess and his wide circle of friends and other political acquaintances. More than a hundred are from the aged and declining Duke of Newcastle, and most of these I believe had never been seen by living historical students.[3] In them and the fewer letters from the Marquess to the Duke lies the story of how Rockingham took up the mantle that slipped from Newcastle's enfeebled shoulders: a story that has never been fully related. And in the letters of the Duke of Portland, the several Cavendishes, Admiral Keppel, Sir George Savile, the Duke of Richmond, William Dowdeswell, John Lee, Sir William Meredith, Charles Fox, General Conway, Sir Charles Saunders, Earl Fitzwilliam—to say nothing of Burke —may be found the inner history of that once-famous but now near forgotten band of virtuous and principled political men called the Rockingham Whigs. A small part of this material was published a century ago by Lord Albemarle in his *Memoirs of the Marquess of Rockingham*, but that odd and ill-arranged compilation is not a history; nor has the inner party history of the Rockinghams ever been taken from the original sources and given to the world. Few of these figures have ever been honored with a worthy biography. Rockingham surely deserves one, and if and when it is done I believe a new conception of him may arise. We know how greatly Burke esteemed him, but we have never taken him at Burke's estimate; and I think Burke has obscured him for us by towering over and casting him into the shadow. But with a more realistic understanding made possible by these papers—many of which are unpublished exchanges between Burke and Rockingham—it may appear that Rockingham taught and inspired Burke, at least in the arts of political prudence, as much as we have always assumed that Burke instructed Rockingham.

I can give but a few more hints of what is in these documents. There are perhaps a score of unpublished letters of Chatham, more from Lord Hardwicke, a few from Charles Townshend, a few of Camden's, some unseen bits from George III. Lord Temple, Lord Holland, Lord George Germaine, Wedderburn, Shelburne, and dozens of others are represented. Of special interest are the letters of Sir George Savile, one of the most enlightened and just political men of that age. His philosophical temper, his candor, and gift for seeing the point of view of his opponent, made him an excellent historical witness. Finally, I would draw attention to the many letters of the Duke of Richmond. He was an odd and puckish political character who never was able to grasp the Rockingham principles but whose heart was always right and honorable, and who had a wonderful gift for writing witty and entertaining letters. A study of Richmond's caprices throws much light on the inability of Rockingham and Burke to make the elements of their party cohere.

Something remains to be said of the Fitzwilliam papers. I do not refer to all the documents illustrative of the history of that family but only to the political papers of the second Earl, nephew and heir of Rockingham. How many these are I cannot say with certainty, but I was able to make a rough inventory of about three hundred items, most of which had to do with Fitzwilliam's appointment to the Irish Lord Lieutenancy in 1794, his unsuccessful effort to carry Catholic Emancipation, and his recall by Pitt and the Duke of Portland in March, 1795. Much of the secret history preceding the rising of '98 and the Act of Union will be found in these documents. Probably the whole correspondence touching the Irish question exchanged between Viceroy Fitzwilliam and Home Secretary Portland is here, along with letters from scores of persons intimately concerned in this dark episode of history; one may find here some of the wisest and noblest letters Burke ever wrote. In the Wentworth papers as a whole, I would estimate that there are perhaps half a hundred letters exchanged between Burke

[3] Professor Randolph G. Adams, of the University of Michigan, has informed me that in 1934 he saw some of these papers and made a few transcriptions.

and Fitzwilliam from 1791 to 1797. Some have to do with Burke's debts, but most of them concern the 1794 coalition and the effort to free the Irish Catholics. Hardly a fragment of this has ever seen the light of publication, and the importance of it may be indicated by the fact that the editors of Burke's correspondence excluded from their volumes every one of these letters. When they were preparing their publication, in the late 'thirties and "hungry 'forties" of the last century, Irish nationalists were agitating for repeal of the union, and the letters were probably thought too hot to handle. They are cooler now and await the hands of scholars who may give us a much better history of Anglo-Irish politics in the age of the French Revolution than it has so far been possible for any one to write.

Such is a rough summary, by no means accurate and perhaps not even proportionately just account of the wealth of these papers. Viewing the whole, one may say that they are the materials for close and intimate study of one of the most remarkable phenomena in political history. In the inscription Burke wrote for Rockingham's monument at Wentworth he said the Marquess had endeavored "to give stability to the liberties of his country, security to its landed property, increase to its commerce, independence to its public counsels, and concord to its empire"; and that his aim through life was

> to convert party connexion and personal friendship (which others had rendered subservient only to temporary views and the purposes of ambition) into a lasting depository of his principles, that their energy should not depend upon his life, nor fluctuate with the intrigues of a court, or with the capricious fashions amongst the people; but that, by securing a succession in support of his maxims, the British constitution might be preserved, according to its true genius, on ancient foundations and institutions of tried utility.

If those words were true, they described one of the rarest of human things: a genuinely conservative political party composed of men who—in better known words of Burke—are "united for promoting by their joint endeavours the national interest upon some particular principle in which they are all agreed." That famous definition of party may be applied to many political associations in our times. It fits the various revolutionary socialist, communist, and fascist parties of strict ideological creed, but it does not fit the great non-revolutionary parties of English-speaking nations that are loyal to, and seek the conservation of the historic constitutional political societies in which they act. It does not fit the party of Senator Taft and Senator Tobey, or the party of President Truman and Senator Byrd; it fits neither the British Tories nor the Laborites. All these parties are conservative in the sense that none proposes—at least not knowingly—to tear up the broad base and structure of the state; all would preserve that; but all are combinations of men and women who have compromised, or pooled, or pragmatically coordinated their interests, instead of uniting to promote "the national interest upon some particular principle in which they are all agreed."

It may be that such a party as Rockingham, aided by Burke, formed and led is as obsolete in a democratic society as the Georgian palace at Wentworth is in an era of pre-fabricated flats and confiscatory inheritance and income taxes. It was formed on completely aristocratic principles, and even in the eighteenth century it could not gain and govern the state; but it generated an immense reservoir of public virtue and political intelligence. Hence it was good, and good things are infinitely adaptable. So that even as Wentworth Woodhouse is today becoming a public school, so may the Rockingham party principle possess a comparable adaptive utility. Lecky said of Burke's writings that the time might come when they would no longer be read, but there could never be a time when men would not grow wiser from reading them. I suggest that the history of Burke's practical party politics, which can now be so copiously documented, must ever be a profitable study for those who would understand the nature of that political animal whose name is Man.

PROBLEMS OF BURKE'S LETTERS[1]

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A FEW of the problems of Edmund Burke's letters one would like now to call "past problems," since they have to do with a situation which no longer exists. For a century and a half the scholars of the English-speaking world did not have access to the main body of Burke's papers. Now they have such access. We should be grateful for our blessings and plunge into the present task.

But I am afraid we may not quite understand what the present task is unless we survey at least briefly a few of the "past problems." A good deal takes place when a historical figure of Burke's stature is inadequately known for a period of a hundred and fifty years. He is not, obviously, ignored; he is only discussed inaccurately and often unintelligently. He is the subject of a great many blurred generalizations, a great many wordy, unconvincing analyses. Biographies of him are written, but they are insufficiently documented. He begins to bore mankind, when it is gradually discovered that those who talk of him never quite have their facts. This, I believe, has partly happened to Burke, and I think it is well for us to recognize how much its happening has been the result of the fate of his papers.

Within a decade, or at most two decades, of Burke's death in 1797 three services should have been done his fame. His works should have been edited, an "authorized" biography should have been written, and his letters, or a generous selection of them, should have been printed. Burke's literary executors, Dr. French Laurence and Dr. Walker King, promised all three of these things, and they were apparently in a position to perform them. They held his papers; indeed they had begun collaborating on an edition of his works while he was still alive. At his death they wrote to his friends to retrieve as many as possible of his surviving letters. These they expected to publish, and also to make the basic materials of a biography.

But many things went wrong in the execution of their triple task. Laurence and King were not congenial collaborators; both were busy men in their own affairs, all too prone to neglect the papers and to blame each other for omissions and delays. When Laurence died, twelve years after Burke, even the edition of the works was only half completed, and neither the biography nor the edition of the letters had been begun. None the less King promised to carry on alone. As he had many other duties—being in fact a Bishop of the Church of England—and as he was also slowly going blind, he was by no means the ideal person for large scholarly tasks. When he died, more than thirty years after Burke, he had only just finished the edition of the works; he had not begun the biography nor printed more than a score of the letters. He had, however, held onto the essential papers long enough to prevent other scholars from working upon them in the period when Burke's surviving contemporaries could have been questioned, when disputed facts in his life could have been established—in a word when it was still possible for an authoritative biography to be produced.

On King's death the custody of Burke's papers passed to the second Earl Fitzwilliam, who had been named by Mrs. Burke as an additional executor of her husband's remains. But Earl Fitzwilliam was by then a man of eighty—hardly ready to rush at the long-neglected task and make up for the time which had been lost. When he died, five years after King, no more had been done with the papers. The body of them remained, however, in the possession of the Fitzwilliam family, and after another delay of nine years the third Earl Fitzwilliam did make some further use of them. In collaboration with Sir Richard Bourke this Earl brought out the four-volume work which most of us know under the title *The Correspondence of Edmund Burke*. It appeared in 1844—that is, nearly fifty years after Burke's death.

This work contains about 450 letters by and to Burke, together with a few other documents il-

[1] Paper presented at the meeting of the American Historical Association in Boston, December 30, 1949.

lustrative of his life and times. Since it is very much the most important collection of Burke letters in print, it is worth our while to pause for a moment to comment upon its virtues, and also to mark its limitations. It is on the whole a careful piece of scholarship. I had many occasions when working at Sheffield last summer to check upon the accuracy of its texts, its notes, and especially its ascriptions of dates. I gained a high respect for the sanity and accuracy of the work. At the same time, the task I was carrying out—that of listing all letters in the Fitzwilliam collections—showed me where its limitation lay. There are, as I say, about 450 letters by and to Burke printed in this edition. The total number of letters by and to Burke in the Fitzwilliam collections is well over 3,000. Obviously the edition of 1844 is far too narrow a selection even of the body of papers upon which it is based.

But we must pay attention to the fact that many Burke letters exist outside of the Fitzwilliam collections. Most of us who have had any occasion to study Burke are aware that the list of his published letters has long since extended itself beyond the number of 450. Doctors Laurence and King when they attempted to call in Burke's letters after his death clearly did not succeed in getting all that were extant, so that throughout the nineteenth century and up to the present time scattered batches of letters have been showing up in all sorts of places, and of course often finding their way into print. Some of these are readily found when we need to consult them, being in volumes which are normally catalogued under Burke's name. Such are Burke's correspondence with French Laurence, published in 1827; his correspondence with William Windham, published in 1910; his schoolboy letters to Richard Shackleton, published in 1923. But there are other bodies of letters one could very easily miss. Who would think that a book with the title *Two Centuries of Ceramic Art in Bristol* would contain one of Burke's most important correspondences? Who would bother to comb Burke's biographies to find the scores of letters printed there and never reprinted in the four-volume *Correspondence?* Naturally the biographies of Burke's friends—based upon *their* collected papers—often contain Burke letters. The publications of the Historical Manuscripts Commission give us the texts of many more. Works like the Catalogue of the R. B. Adam Library or the Catalogue of the Duke of Portland's Library add more. Isolated letters and small groups have been published here and there in periodicals.

I have been going into what may seem excessive detail in reviewing these published letters, for I believe that it is important that you realize the great difficulty one encounters at present in using that part of Burke's correspondence which *is in print*. Printed letters we must classify as accessible, and this means that patient scholars will contrive to use even the widely scattered materials I have been describing. But anyone who forms the purpose of using the full body of Burke's correspondence now in print will have to supplement the four-volume edition of 1844 by consulting at least two hundred other volumes.

One turns eagerly from the enumeration of such difficulties, which I should be glad to call the "past problems" of Burke's correspondence. The present situation would seem to be much simpler. We now have access to the great mass of Burke's papers. Let us consider the availability of Burke letters in manuscript.

First, the Fitzwilliam collections. These present us with one minor problem which I have not yet mentioned. The Fitzwilliam Burke papers are not all in one place. About four-fifths of them, those owned by the present Earl Fitzwilliam, are on loan in the Sheffield Public Library. But by some accident of time another fifth, or approximately that, has been separated from the main bulk and is at present owned by a member of a collateral branch of the family living at Milton near Peterborough—Captain Thomas W. Fitzwilliam. Captain Fitzwilliam, like the Earl, has recently placed his papers on loan, but not in the Sheffield Public Library. His are in the Northamptonshire Record Society at Lamport Hall, Northamptonshire. The curators of this institution are extremely courteous and enlightened, and if I can judge by my stay in Lamport this summer they make it a pleasant experience for any scholar to work in their fifth of the Burke papers. But it is undoubtedly an embarrassment to have to work upon one-fifth of a body of papers in one place and upon the other four-fifths in another place one hundred miles away. And much as I should like to believe it, I do *not* believe that the curators at Lamport will be willing to relinquish their hold upon their fifth of the papers in order to see them reunited to the main bulk at Sheffield.

One studies the manuscript letters of Burke, then, first at Sheffield, second at Lamport. These two collections together total about 3,000 letters

by and to Burke. But then one must look about for something between 500 and 1,000 other manuscript letters which are accessible but not in either of these collections. As we have already noticed, Doctors Laurence and King did not succeed in laying their hands on all existing correspondence of Burke. Some batches which escaped them found their way into print. We know of others still in manuscript. Professor Hoffman has just mentioned the collection of Burke's letters to Charles O'Hara, now held by the O'Hara family in County Sligo, Eire. This is a large collection, none of which is in print. A comparably large collection, partly printed, is held by a gentleman at Colne, Lancashire. There is a large group of Burke letters in the British Museum, most of which have been published. There are unpublished letters in numbers from a couple dozen down to one or two to be found in virtually every important scholarly library in the English-speaking world. The Bodleian has some, the Fitzwilliam Museum at Cambridge has some, the India Office Library, the National Library of Ireland, Trinity College Library—all have some. In America there are a couple dozen in the Pierpont Morgan Library, about the same number in the Huntington, Yale now has the bulk of the Burke-Boswell correspondence, a few Burke items are in the New York Public Library, the Boston Public Library, the Harvard, the Clements, and so on. Private collectors have still more, and of course some are still in the hands of autograph dealers.

To summarize all this: we now know of the location of something like 3,800 letters by and to Burke which are accessible in manuscript. They are not so dreadfully scattered, perhaps, as the printed letters we have just reviewed. They are in scores rather than hundreds of separate places. But they too show us the present limitation of that four-volume work called *The Correspondence of Edmund Burke*, with its 450 letters. If we put together all our known Burke letters, printed and in manuscript, we shall find that there are well over 4,500 in all. In other words the edition of 1844 contains about one-tenth of Burke's surviving correspondence.

(May I indulge in an "aside" at this point? I fear I may have troubled some of you by stating these totals always in the doubled form of "letters *by and to* Burke." When dealing with the Fitzwilliam collections, at least, it is natural to state totals in this way, for of course the body of Burke's own papers is made up preponderantly of letters *to* Burke. You may be curious to know how many letters *by* Burke there are in the 4,500 I have just mentioned. My most recent count brings the total of known letters *by* Burke to over 1,600. Of this 1,600 the four-volume edition of 1844 prints 266.)

Now I do not have to convince a roomful of historians that it would be a very desirable thing for us to have a fresh edition of Burke's correspondence. I take it for granted that this is so. A world government which was of historians, by historians, for historians would make an immediate grant of funds for such a project. A competent editorial committee with nothing else to do would then go to work gathering together the scattered letters I have been describing, and in perhaps three years time we should have clear authoritative texts of all the letters within one set of covers and with one inclusive index. This would not have to be any such scholarly showpiece as the Horace Walpole Correspondence now being done at Yale or the Thomas Jefferson Correspondence now being done at Princeton. It would be a sound, useful work, running probably somewhere between eight and twelve volumes, and involving an expenditure before it got into print of perhaps as much as $50,000.

Now, if I were addressing a roomful of exceptionally well-disposed millionaires perhaps I should end my remarks at this point. Since it would be the clear duty of those millionaires to produce the $50,000, it would be bad for them to suppose even for a moment that an edition of Burke's letters could be delayed in its emergence. It should not be; I believe we can all agree upon that. But you as historians, not millionaires, know that sometimes the most necessary projects *are* delayed in the execution, and therefore a discussion of my topic "Problems of Burke's Letters" may as well deal with the question of what historians are to do if they are *not* presented with a new edition of Burke's letters in less than five years. I assume that work on Burke will go on, even under difficulties. But how can we minimize those difficulties, and are there any steps which we can take to bring ourselves slowly out of the present chaotic situation?

I have already ventured upon one step which I think is in the right direction. In collaboration with Professor Milton Smith of Hillyer College, Hartford, Connecticut, I have made a survey of all accessible Burke letters, in print and in manuscript, in order to incorporate the essential reference information about them into one great list.

I describe this as one list, but when completed it will actually be two: one a straight chronological arrangement of all known letters by or to Burke; the other an alphabetical series of Burke's correspondences, with each separate correspondence treated as a unit and arranged chronologically. Burke corresponded with about 1,200 persons we know of, including most of the conspicuous political and literary figures of his age.

This project is not far from complete. It has already been approved for publication by the American Index Society, and is expected to come out in the spring of 1951.

One result of the publication of such a list will of course be that scholars can see what has been done and what is to be done in the matter of printing Burke letters. It will be evident what manuscripts are in what places, and, roughly, how available they are for publication. If the worst happens and we do not get any definitive edition of Burke's letters for a decade, there is no reason that the letters which are lying unpublished in the Morgan Library, the New York Public Library, the Harvard, the Clements, the Huntington, should not be got at and used. Even small groups of Burke letters have gained importance from the fact that the main hoards at Sheffield and Lamport have now been opened.

This sort of small project of publication would be definitely useful, and yet would involve no great investment of scholarly time. Many of us could suggest such projects to graduate students, or to junior scholars looking for limited undertakings. For mature scholars who could attempt more, and who could get over to Sheffield and Lamport, Dublin and Sligo, there are tasks of greater importance. Professor Hoffman has given us specimens of such tasks, in what he has said of the O'Hara letters, and the correspondences of Burke with Lord Rockingham, Earl Fitzwilliam, or the New York Assembly. To do a sound job of editing for any one of these correspondences might occupy a mature scholar for months or even for years, and yet they are in one sense *limited* projects—that is, well-defined in scope, and hence practical.

It is possible that it will be by a series of individual efforts of this sort that we shall ultimately cope with the great mass of Burke's correspondence. Many of us would regret such random methods, and much prefer one large and well-organized attack upon the entire problem. But I believe we should be prepared for either event—prepared to do the work in any way that proves practicable. All we should insist upon is that the work be done, and that the study of Burke which has been so long neglected should be pursued eagerly now.

"INANNA'S DESCENT TO THE NETHER WORLD" CONTINUED

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(*Read November 5, 1949*)

FEBRUARY 1942, a little over eight years ago, it was my privilege to publish in the *Proceedings* of the American Philosophical Society the contents of a Sumerian myth entitled "Inanna's Descent to the Nether World." [1] The extant text of the myth had been pieced together from thirteen tablets and fragments inscribed about 1700 B.C., more than 3,500 years ago. All the pieces were excavated at the ancient Sumerian site Nippur, not far from modern Baghdad, and are now located in the Museum of the Ancient Orient in Istanbul and in the University Museum in Philadelphia. As the paper pointed out, the text of the poem was still incomplete, and it expressed the hope that sooner or later some of its missing portions would be uncovered and translated. The present paper is a preliminary report of the first if, let it be stressed, partial, realization of the hope. A tablet inscribed with part of the "Inanna's Descent" myth, the contents of which were not utilized at the time, is now in the Yale Babylonian Collection in New Haven [2]; it had been bought some decades ago from a dealer, and its provenience is therefore unknown, but it may well have been Nippur. The tablet is excellently preserved and contains ninety-two lines of text. The first sixty-two lines add relatively little since they duplicate a portion of the text already known. But the last thirty lines are invaluable; they contain an entirely new passage which carries on the story from where it had broken off in the previously known texts. Moreover this new material has a rather unexpected significance; it clears up a misconception concerning the Sumerian deity Dumuzi—the Biblical Tammuz—which has misled students of Mesopotamian mythology and religion for more than half a century.

First, however, in the words of Pharaoh's cupbearer, "Let me mention my sins today." For I should have known and utilized the contents of this Yale tablet when reconstructing the text of the myth in the nineteen-thirties, since the existence of this tablet in the Yale Babylonian Collection was known and noted as early as 1924 by the late Edward Chiera who had copied many Sumerian literary texts in Istanbul and Philadelphia. But unfortunately his very brief notation of its existence in the introduction to his volume entitled *Sumerian Religious Texts* [3] escaped my attention at the time. But some years ago I had the opportunity of visiting the Yale Babylonian Collection, and, with the cooperation of its curator Ferris Stephens and his colleague Albrecht Goetze, helped to catalogue its over one hundred Sumerian literary tablets and fragments. It was in the course of this work that the contents of the "Inanna's Descent" tablet and its significance for the myth were recognized. Ferris Stephens has since made a faithful and careful copy of the tablet which he forwarded to me, and I have now prepared a tentative transliteration and translation. The final scientific publication of the tablet, which is to appear in the *Journal of Cuneiform Studies,* will be a cooperative study under the joint authorship of Ferris Stephens and myself.

Before proceeding with the sketch of the contents of the new passage, it will be advisable to review ever so briefly the plot of the myth as it was available eight years ago. For some unknown reason Inanna, queen of heaven, the Sumerian goddess of love and war, has set her heart upon visiting the nether world. She therefore collects all the appropriate divine decrees, adorns herself with her queenly robes and jewels, and is ready to enter the "land of no return." Queen of the nether world is her older sister, and seemingly bitter enemy, Ereshkigal, the Sumerian goddess of death and gloom. Fearing lest her sister put her to death in the nether world, Inanna instructs

[1] *Cf.* the writer's Sumerian literature: a preliminary survey of the oldest literature in the world, *Proc. Amer. Philos. Soc.* **85** (3): 293–323, 1942.

[2] The tablet is catalogued as YBC 4621.

[3] *Cf.* p. 38 of the introduction.

her messenger Ninshubur, who is always at her beck and call, that if after three days she shall have failed to return, he is to set up a hue and cry for her in heaven, in the assembly hall of the gods. He is then to go to Nippur, the city of Enlil, the leading god of the Sumerian pantheon, and plead with the latter to save Inanna and not let her be put to death in the nether world. If Enlil refuses he is to go to Ur, the city of the moon-god Nanna, and repeat his plea. If Nanna, too, refuses, he is to go to Eridu, the city of Enki, the god of wisdom, and the latter who "knows the food of life," who "knows the water of life," will surely come to her rescue.

Inanna then descends to the nether world and approaches Ereshkigal's temple of lapis lazuli. At the gate she is met by the chief gatekeeper who demands to know who she is and why she has come. Inanna concocts a false excuse for her visit, and the gatekeeper, upon instructions from his mistress, leads her through the seven gates of the nether world. As she passes through each of the gates her garments and jewels are removed piece by piece in spite of her protests. Finally after entering the last gate, she is brought stark naked and on bended knees before Ereshkigal and the Anunnaki, the seven dreaded judges of the nether world. These fasten upon her their eye of death and she is turned into a corpse which was then hung from a stake. So pass three days and three nights. On the fourth day Ninshubur, seeing that his mistress has not returned, proceeds to make the rounds of the gods in accordance with his instructions. As Inanna has predicted, both Enlil and Nanna refuse all help. Enki, however, devises a plan to restore her to life. He fashions the *kurgarrû* and the *kalaturru*, two sexless creatures, and entrusts to them the "food of life" and the "water of life," with instructions to proceed to the nether world and sprinkle this "food" and "water" sixty times upon Inanna's impaled corpse.[4] This they do, and Inanna revives. As she leaves the nether world, however, and reascends to the earth, the resurrected goddess is accompanied by the harpies and bogies who make their home in the lower regions. Surrounded by this ghastly demoniac company Inanna first proceeds to visit the two Sumerian cities Umma and Badtibira.[5] The tutelary deities of these cities, Shara and Latarak by name, are terrified at the sight of these unearthly arrivals; they clothe themselves in sackcloth and grovel in the dust before Inanna. Inanna seems to be gratified by their humility, and when the demons threaten to carry off these deities to the nether world she restrains them, and thus saves their lives.

Here is where the text of the myth broke off eight years ago, and it is the Yale tablet which now enables us to continue the story, as follows: After her visit to the first two cities, Inanna and the demons continue their journey and arrive at a Sumerian city with the partly illegible name . . . . —Kullab. The tutelary deity of this city is the shepherd-god Dumuzi, or, as the name is usually pronounced, Tammuz.[6] This god Dumuzi, according to Sumerian mythology, is actually Inanna's husband, and it is perhaps not surprising therefore to find him refusing to wear sackcloth and grovel in the dust before his spouse. In any case upon the arrival of Inanna and the demons he dresses up instead in festive array and sits loftily upon his throne. Enraged, Inanna looks down upon him with "the eye of death" and hands him over to the eager and unmerciful demons to carry him off to the nether world. Whereupon Dumuzi turns pale and weeps. He lifts his hands to the sky and pleads with the Sumerian sun-god Utu, who again, according to the Sumerian mythological notion, is his wife Inanna's brother, and therefore his own brother-in-law. He begs Utu to help him escape the clutches of the demons by changing his hand into the hand of a snake, and his foot into the foot of a snake. And here unfortunately, right in the middle of Dumuzi's prayer to Utu, our Yale tablet comes to an end, and we do not even know whether or not Utu heard Dumuzi's plea or whether the latter was actually carried off to the nether world. And once again we are compelled to express the hope of eight years ago that some day the clay tablet on which the end of the story is inscribed will be uncovered.[7]

[4] There were a number of additional instructions which the *kurgarrû* and *kalaturru* had to follow, but they are of uncertain character since the text is badly broken at this point.

[5] Immediately upon Inanna's ascent from the nether world and before she proceeds to visit the various Sumerian cities she is met by her faithful messenger Ninshubur garbed in sackcloth and groveling in the dust. In spite of his efforts which helped to save Inanna's life, he is threatened by the demons, but is saved by Inanna.

[6] This pronunciation follows the Biblical transliteration of the name; *cf.* Ezekiel 8, 14.

[7] A joint expedition of the Oriental Institute of the University of Chicago and the University Museum of the University of Pennsylvania, is now conducting excavations at Nippur, the place where most of our Sumerian literary material comes from. And so, while it is never wise to predict, there is some possibility that the hope expressed in this paper may be realized at least to some extent in the not too distant future.

Now as already noted, brief as the new material added by the Yale tablet actually is, it has a rather surprising significance; it helps to straighten out a mythological misconception which has been current among Orientalists for many a decade, almost ever since the Semitic version of our myth, known as "Ishtar's Descent to the Nether World," was first published,[8] and long before its Sumerian counterpart came to be known to any significant extent. For all along it has been generally supposed that the god Dumuzi had been carried off to the nether world for some unknown reason some time before Inanna's descent to the nether world. Indeed it was this assumption which seemed to provide the motivation for Inanna's descent; that is, it was generally supposed that Inanna descended to the nether world in order to free her husband Dumuzi and bring him back alive. The new Yale text, however, shows that these assumptions were quite groundless. Not only did not Inanna save her husband Dumuzi from the nether world, it was she who, angered by his contemptuous attitude, actually handed him over to the nether world demons to carry him off to the land of no return.

We conclude with a tentative line by line translation of the new material on the Yale tablet. The first line contains the speech of the demons who together with Inanna are now in the City Badtibira where Inanna had just forbidden them to carry off its tutelary deity Latarak who had humbled himself before Inanna by putting on sackcloth and sitting in the dust; the disappointed demons then say:

"Let us accompany thee to Kullab, the. . . ."

They accompanied her to Kullab, the . . . ;
(There) Dumuzi dressed himself in a noble garment,
seated himself loftily on (his) seat.
The demons seized him by the anus,
Poured out the seven *table-drinks*,[9]
The seven (demons) attack him like the *strength* of the sick,
The *shepherds* play not the flute and the pipe before him.

She fastened the eye upon him, the eye of death,
Spoke the word against him, the word of wrath,
Uttered the cry against him, the cry of guilt:
"As for him, carry him off."
The pure Inanna gave the shepherd Dumuzi into their hands.

They who accompanied him,
They who accompanied Dumuzi,
(Were beings who) know not food, know not water,
Eat not sprinkled flour,
Drink not libated water,
Sate not pleasurably the lap of the wife,
Kiss not the *honey-fed* child;
They lifted the man's son from his knee,
They carried off the bride from the house of the father-in-law.

Dumuzi wept, his face turns pale,
Towards heaven, to (the sun-god) Utu, he lifted his hands:
"O Utu, thou art my wife's brother, I am thy sister's husband,
I am one who brings fat to thy mother's house,
I am one who brings milk to Ningal's house,[10]
Turn my hands into the hands of a snake,
Turn my feet into the feet of a snake,
Let me escape my demons, let them not seize me."

[8] For details *cf.* now, Alexander Heidel, *The Gilgamesh epic and Old Testament parallels,* 119 ff., Chicago, Univ. of Chicago Press, 1946.

[9] All italic words are doubtful renderings.

[10] According to Sumerian mythology, Ningal, the wife of the moon-god Nanna, is the mother of Utu, *cf.* the writer's *Sumerian Mythology, Mem. Amer. Philos. Soc.* 21: 74, 1944.

BABEL IN MEDICINE

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(*Read April 21, 1950*)

THE AMERICAN PHILOSOPHICAL SOCIETY is unique among learned bodies in its intentional interest in all branches of knowledge. Its earliest meetings heard reports on "the electric," the transit of Venus, a recipe for making wine from American grapes, and a model of a plough. The programs ever since have revealed the same diversity of topic, always with the common purpose of the advancement of useful knowledge.

Perhaps the words *"nullo discrimine"* on the seal of our Society refer to this absence of discrimination against any branch of knowledge, although some have suggested that the phrase merely reflects the liberal spirit of the seventeen seventies. Again this Latin tag might indicate that there was no prejudice between the two groups which merged to form the American Philosophical Society. The minutes are silent on this point and although we know that *"nullo discrimine"* was a favorite phrase of Vergil occurring four times in the *Aeneid*, we find no peculiar use in any of these which would explain its choice by the committee which designed our seal.

In those early days it was not difficult for our members to understand all of the presentations made at the meetings. The matters were not too complex; knowledge was not too specialized. Terminology had not yet become the iron curtain which today tends to prevent intercourse between different groups of scientists of even related fields. The electric was then, of course, a new term derived from the Greek word for amber because of the phenomena observed when amber was rubbed. Thus, all our electric terminology will be "forever amber."

Today scientific terminology is a serious problem to everyone, and to the American Philosophical Society this problem is especially acute because of our catholic interests. This matter can be well illustrated by reviewing the factors which have led to the present babel in medical terminology.

Much of our medical glossary is first found in the writings of Hippocrates although, of course, he merely employed the current usage of his period including such modern sounding words as arthritis and pneumonitis of Greek origin and also foreign words such as the Egyptian hyalos meaning glass and the Cretan meninx, from which come our terms hyaline and meningitis. Some of these ancient medical words continue in use today with unchanged meanings as for example nausea, the nautical or ship disease. But many others have undergone such changes in form or meaning as to be scarcely recognizable. The source of some words, such as abdomen, is not known today. In other instances the reason for a word is almost forgotten.

Take, for example, the familiar word carotid—the name of the large artery in the neck. The word carotid is derived from the Greek, karoun, which meant to stupefy or put to sleep. There is an illuminating story back of this unexpected derivation. In ancient Greece the peripatetic mountebanks as a feature of their shows caused a goat to fall unconscious by pressure on the carotid arteries in the neck, and to return to normal when the pressure was relaxed. This story was given credency by the famous Morgagni (1682–1771), but the anatomist, John Bell, later scoffed at him for doing so. Still later, Astley Cooper and others, however, demonstrated that in goats and horses, but not in man, the carotids supply the entire blood flow to the brain; and in 1896 Leonard Hill in his book on the cerebral circulation wrote, "So in spite of Bell's scoffs, the mountebanks were in the right." And so, of course, was Morgagni.

A medical dictionary contains about 50,000 words, and, as we will see, the derivation and meaning of many of these cannot be surmised from the words themselves. No wonder our specialized speech is a mystery and a joke to the laiety, and a problem to the doctors themselves. One reason for this huge vocabulary is the double nomenclature which resulted when, in the second century

B.C., the Greek physicians of the Alexandrine school began to invade Rome and introduce their Greek terminology.

The Greek invaders largely conquered the native Latin medical terms but as is so often the fate of conquerors, they were absorbed and changed in the process. The Greek word, hemicranion, half the head, became hemicranium in Latin; in France the same process made it demicraine, then migraine, and when it crossed the channel after William the Conqueror, it was anglicized to "the megrims." In Rome the foreign Greek sounded more learned, just as today ophthalmologist sounds far superior to oculist. One can visualize the Greek physicians "putting it over" on the local doctors with a Greek diagnosis of nephritis, but not bothering in anatomy to Hellenize the Latin term ren, the Kidney. We still use many Latin words for anatomical structures, but when one reviews the names of the diseases of these structures one finds a predominance of Greek roots— the portal vein is so named from the Latin porta, a gate or door; but the inflammation of this same vein is pylephlebitis from the Greek pyle, also meaning a gate or door.

We use the word muscle so often we forget to ask its derivation, and we fail to enjoy its overtones. Lost in the past is the story of the first use of the Greek word mus, meaning mouse, also to mean muscle. One can imagine a muscular Greek rippling his biceps and saying "it looks like a mouse running up and down under my skin." Then, we find musculus, a little mouse, in Latin and later in French. Today we use the Greek combining form myo- as in myositis, and also the English word muscle taken from the French. The word muscle appeared in English four hundred years ago—about a century before the Greek root myo- was employed to coin new scientific terms.

Hundreds of such duplicate derivations—one Greek, one Latin—cumber our medical verbage; for example, pneumonia and pulmonary, hysteria and uterus; and in dentistry two parallel sets of terms, one form with the Greek odont, a tooth, as in exodontist, the other from the Latin, dens, as in dentist. Of course, some Latin terms like an underground movement resisted the invaders. These were mostly simple words like lumbago, vertigo, and some earthy terms and they continue in use unchanged.

Other ancient words, however, had their meanings changed as time passed and this has added to the confusion. For example, the word arachnoid, which actually means like a spider or spider web, was applied by Hippocrates to scum on urine, but Galen shifted it to a plexus of nerves and only in the middle of the seventeenth century did it acquire its present meaning of a membrane over the brain. The term organ meant a tool, and hence mechanical; but Plato shifted it to an organ of sense and from this it was applied to the organs of the body and thus came to denote living as opposed to dead, as in organic chemistry. The word vegetation followed a reverse course.

Still further confusion has arisen from mistakes in translation. Many of these crept in when the writings of the fathers of medicine went, so to speak, out of print during the Dark Ages and later had to be recovered by translation from the Arabic copies. Such mistakes in translation are well illustrated by the story of our word for the back of the eye, the retina. To this structure Galen had applied the word amphiblestron, which unfortunately then had two meanings—a surrounding coat and a fisherman's net. Galen used the word in the first sense, but later translators into Latin chose the second and so used the root of the Latin word rete, a net, for this structure, and this error is permanently established in our medical vocabulary.

Another amusing example is found in the history of the word astragalus which today is the name of a bone in the foot. It started off in Greek to mean a vertebra, or bone, in the neck of sheep. How it moved from the neck of sheep to a bone in the foot of man is interesting. The Greeks of the *Iliad* carved their dice from the astragalos of the neck of sheep and so the term came to designate one of a set of dice. The Roman soldiers much later took over the word with this latter meaning but applied it to the heel bone of horses from which they were accustomed to carve their dice—and from this it came into human anatomy. Whichever bone was used the resulting dice must have been very uneven, and to risk much on the rolling of such "bones" might prove expensive.

Sill another example is the word autopsy. Today it means a post mortem examination, but according to its derivation it comes from autos, self, and opsia, to look at. This is one thing one cannot do at one's autopsy. Actually in the third century B.C. the term autopsia denoted those observations on a patient made by the physician himself, in contrast to the historia supplied by the patient, probably just as verbosely then as today. Such examples could be multiplied many times.

All through the years, as medical words travelled from one country to another, erosions and cor-

ruptions occurred. For example, adjectives were used as nouns after the associated noun had been dropped. In ancient times, the arteries were thought to contain air and so got their name. Aristotle called the windpipe a rough artery, but the word arteria was omitted and so the windpipe became the trachea which merely means rough.

With the renaissance of science in the sixteenth century, new coinages began to appear and this has continued ever since in increasing numbers. As new facts and knowledge were achieved, new terms were needed and had to be found in classical Greek or Latin or coined from these languages. It was to Greek that recourse was more often had, partly because this tongue with its many prefixes and suffixes lends itself to the building of new words. These new coinages are now mingled with ancient terms in our vocabulary and it is often difficult to tell one from the other.

Many of these neologisms have been poorly chosen, but once launched the word becomes established and often starts a family of related terms. To illustrate this let us review the history of the word epithelium, which today is becoming a familiar name for the skin although if you should look it up in a dictionary you would find that it is derived from the Greek epi meaning upon, and thele meaning the nipple. It all started about 1700 when an investigator named Ruysch was studying the surface of the tongue and found a layer of cells covering the little papillae which stud the tongue. Wanting a name for this layer of cells he turned, as most did and still do, to the Greek and the best he could do was epithelium although thele actually meant the nipple.

A hundred years later, similar cells were found within the body by His and these he named endo-epithelium with the epi omitted. So now there was a word meaning within the nipple. Still later came mesothelium, epithelioma, endothelioma, and many other descendants of the neologism coined by Ruysch. Very few of those using these terms know or have ever questioned their derivation. This sort of thing is still happening all the time and helps to confuse our terminology.

In the early days of the seventeenth century the Dutch chemist, van Helmont, originated the word gas. He did not know much about the various gases, and he confused the matter by coining another term, blas, for ferments which were supposed to control the action of each gas. The word blas has disappeared. Van Helmont centered the control of gases in the pit of the stomach and one might be tempted to think that he coined the word gas from the Greek gaster = stomach. However, the fact is that he formed it from the Greek chaos = confused, for he suspected that a gas was a confused state of primordial matter. This sort of derivation certainly adds to the general confusion and chaos in medical terminology.

The story of the bacteriological vocabulary is illuminating in this connection. It is less than three hundred years since bacteria were first seen and just a hundred years since the word bacterium appeared in English. With the development of this science a whole new vocabulary was required and no help was to be found in Hippocrates, Celsus, or Galen. So it was from lay sources in Greek and Latin that assistance was obtained. As we study these derivations we can visualize the step by step advance of bacteriology.

The bacteriologist grows the germs in cultures and culture is an old Latin word direct from the farm. Descriptive terms were chosen for various forms of bacteria; coccus was a berry, a bacillus was a little rod. Strepto- we get from the Greek word meaning twisted—but it came to be applied to jewelry of twisted metal, and so to mean a chain, in which sense it was adopted into bacteriology for the streptococcus which grows in chains. Staphyle, as in staphylococcus, was nothing but a bunch of grapes. Microbe, however, was coined by Sedillot, a French surgeon of the nineteenth century. Germ comes from germen meaning to sprout.

Toxin, of course, comes from the Greek meaning a bow. An arrow poison was called toxikon pharmakon, pharmakon meaning drug, but this latter word was eventually omitted and toxikon was latinized as toxicum = a poison. In 1886, the word toxin was coined to meet a need in modern scientific terminology. From the same ancestry, of course, comes the term toxophily, which is the name given to their sport by addicts of archery.

On the other hand, the analogous Latin word virus meant a slimy liquid, a poison, or even an offensive odor or taste. It was adopted into English over 350 years ago and amazing as it seems was used prophetically in 1800 of the virus of smallpox; a very long time before it was discovered that smallpox was caused by one of the agents which have come today, for lack of a better term, to be called a virus. It is interesting to reflect on the long course which brought these two words, one Greek, one Latin, both meaning poison, from their primary use, to their extremely limited significance today. A toxin and a virus

both originally poisons, but how very different today.

One could develop this approach much further—monilia, the name applied to certain yeasts which grow like a necklace, means a string of beads. The bacteriologist grows his germs in an incubator; incubate means to lie in, or on, and so is related to the male incubus or nightmare which lies on you in contrast to the female succuba which lies under. It is historically interesting that this word incubatio was also in use in ancient times to denote the practice of sleeping in the sanctuaries of Aesculapius in the hope of being cured of disease. We forget the origin of words as familiar even as inoculation. This also came from the Roman farms where inoculatio meant nothing more than a graft. Our medical dictionaries simply state that it comes from in = in, and oculus = eye. In the eye is not very clear unless we remember the eye of a potato and the procedure of grafting—equally applicable to a fruit tree or to the introduction of a disease agent into a human for the purpose of immunization. All this terminology is so easily understood if the derivations are known, but if it has to be memorized parrot-like it is confusing. Similarly, if we do not remember the common origin of a group of related words we can only know each of them separately. For example, the words pupa, pupil in both its uses, pup, and puppet all come from the Latin pupa, a doll or a little girl. The use of pupa for an early stage in the life of an insect is obvious as are pup, puppet, and the pupil at school; but the transfer of the term to the opening in the iris of the eye needs the explanation that it came about from the small reflections one sees when one looks into another's eye.

In each of the various subspecialities of medicine, the terminologies present different types of confusion. The parasites present a formidable list of names which have been frequently changed since 1758 when the tenth edition of Linnaeus' famous *Systema Naturae* included them. Parasite, however, continues to mean a toady or guest who sponges off the rich and like "the man who came to dinner" stays on and on.

Dermatology has many ancient names of diseases, but in many instances these names are applied today to quite different skin disorders than formerly. Neurology has some tongue twisters; surgery has many polysyllabic terms denoting the complicated miracles of modern surgical operations. Chemistry adds its share to the general babel with long terms which are, however, often easily broken down into the component building blocks. It is interesting that already there is confusion about the origin of as recently coined a term as barbiturate—some deriving it from the Spanish moss, *Usnea barbata,* from which it is said to have been extracted; others from the bit or bart of a key, for barbituric acid is the key to a series of chemical substances called ureides. Already this term is adding confusion and now its pronunciation is changing further to hide its identity.

Identical words are used in different fields with very different meanings: the chemist speaks of a molar solution, the dentist of a molar tooth; the physician of sprue as a disease, the dentist of sprue as the ingate into a mold. And so it goes, the babel getting worse as the years go on instead of better.

We are supposedly discussing the threat of scientific terminology, but it is hard for me not to get carried away by my interest in medical etymology for it is fascinating and turns up all the time. You visit your dentist and he tells you he is going to test the pulp of one of your teeth with an electrical vitalometer. At once you explain to him that Cicero limited the use of the word pulpa, from which pulp comes, to the soft flesh of the breasts and buttocks of young maidens, and that for testing the sensitivity of these, it seems to you that there are better and more primitive methods than a vitalometer.

Once you start on this hobby you find new and entertaining derivations every day. In Rome a tonsilla was a mooring post for boats, standing out from the bank of the Tiber; today a tonsil stands out from the side of our throat and is a happy mooring place for passing streptococci.

The word idiot has an ancient ancestry back to a Greek word meaning an ordinary or lay person, one not holding office—from this it came to mean a simple or uneducated individual and finally one who is feebleminded. Today it almost seems that we are all idiots for not getting on the governmental payroll, but perhaps we soon will be whether we like it or not.

The more of such derivations one knows the easier it is to understand what otherwise may sound like medical gibberish.

This problem of confusion in scientific terminology presents a serious threat to all who teach in any scientific field for they must realize that the student of today has, as a rule, had little Latin and no Greek with the result that terminology is a tremendous stumbling block to many students.

I have used medicine as an example, but the problem is present in every specialized field. In the not far distant future, there may be many members of the American Philosophical Society who will have no great familiarity with Latin and Greek. When that time comes, specialized terminology will be an even more serious difficulty at the meetings of our society than it already is today. Our Society is pledged to a broad interest in all departments of useful knowledge and this should never be abandoned. It will become more and more difficult for our speakers to present their complex subjects without using words unknown to the audience. We should all appreciate this problem and do what we can to encourage the continued teaching of Latin and Greek, to stimulate interest in scientific etymology and to urge simplicity in scientific speech as far as this is possible.

ARCHAEOLOGICAL RESEARCH IN THE JOHN DAY REGION OF NORTH CENTRAL OREGON[1]

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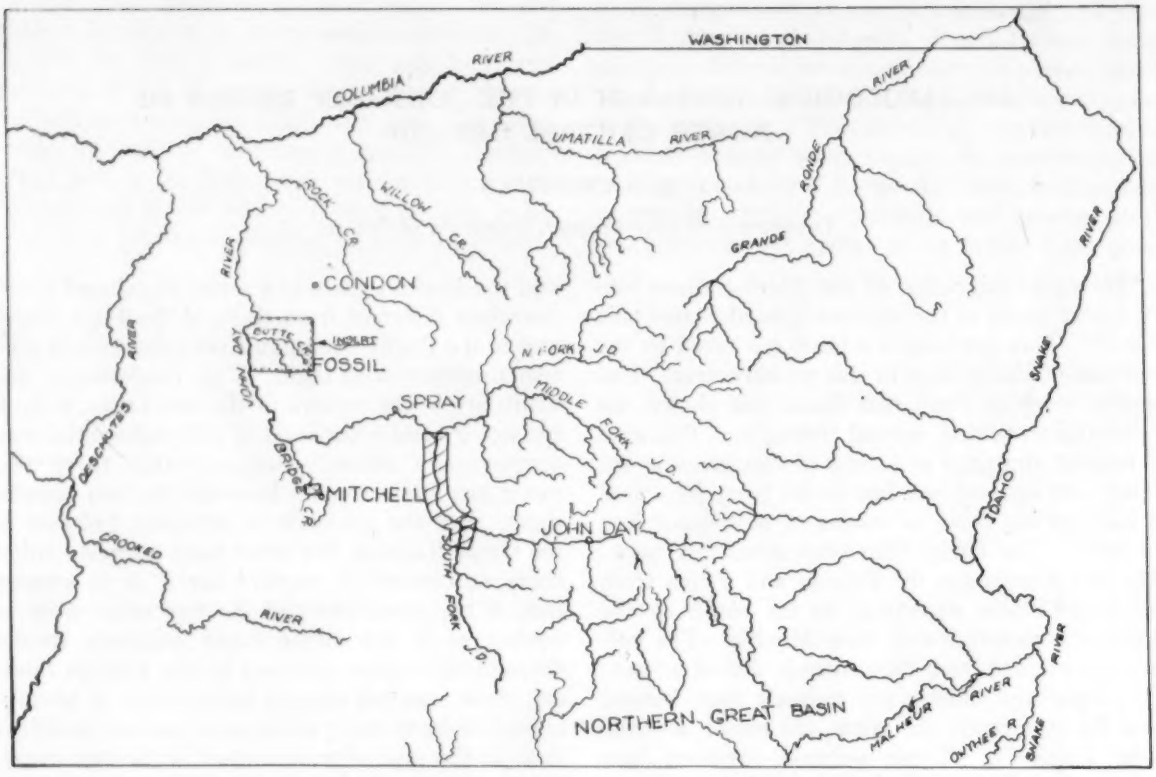
"How did migration of the Paleo-Indians into the Great Basin of the western United States take place?" This question is a basic one faced by the students of Early Man in this western area. Extensive work in the Great Basin has shown the Paleo-Indian widely spread throughout this area of interior drainage at a time of considerable antiquity yet no evidence has so far been presented to indicate his route or routes of movement into the area. The Early Man sites which are probably best known are the Folsom and Yuma from the High Plains extending as far south as the Guadalupe mountains of New Mexico. The culture represented from these sites is that of a hunting people and among the animals they hunted were the mammoth, the horse, the camel, and the bison (*taylori*). These animals survived into early postglacial time and the association of cultural remains with the bones of these animals gives a reliable indication of age. In the Great Basin we have the same basic association though *Bison taylori* has not been reported but other animals such as the sloth have been. The association in the two areas with a similar fauna would suggest a close contemporaneity.

The cultural remains of the Great Basin sites show a mixed hunting and gathering economy, in contrast with the pure hunting economy of the High Plains, where grinding tools are completely lacking. Furthermore, the characteristic Folsom points for all practical purposes do not occur in the Great Basin except as erratics, perhaps intrusives from the area of which they are so characteristic. Yuma points occur with somewhat greater frequency than do Folsoms but they are never characteristic of any Great Basin site. From Central Oregon southward to the Mexican border between the Cascade-Sierra Mountains and the Rockies there is a series of cultural manifestations different from those of the High Plains east of the Rocky Mountains yet substantially contemporaneous with them. This fundamental dissimilarity in the culture of the two areas, both in technology and economy, but with substantial contemporaneity strongly suggests that there was never any basic contact between the two populations. But the presence of intrusive Folsoms in the Great Basin on the other hand suggests either trade or contact of another sort. It is strange that, if the populations of the two areas were in contact or if the Great Basin migrants passed through the region occupied by the Folsom hunters, there was not enough interchange of technological habits to bring about some mutual modifications in the type of points used in the two areas. The truth is that just as the High Plains types have never to any degree appeared in the Great Basin so is the opposite the case. Sayles and Antevs (1941) discussed briefly this problem of entrance and Cressman (1942) also called attention to it. Outside of these two cases of recognition of the problem it has gone without notice in the literature.

POSSIBILITIES OF ENTRANCE OF THE PALEO-INDIAN INTO THE GREAT BASIN

There are three possible routes by which the earliest inhabitants of the Great Basin might have arrived. The first of these is by a direct southward line of movement over the passes into British Columbia between the Cascade and Rocky Mountain ranges. Then they would have been able to move southward along river and lake drainage systems to the Columbia River. Then southward movement into the Great Basin was easy enough by moving up the rivers draining into the Columbia. The passage on beyond the headwaters of these rivers into the Great Basin would have logically followed. The use of this line of migration, however, depended upon the availability of an ice free passage over the northern passes.

[1] Grateful acknowledgment is made to the American Philosophical Society for a grant-in-aid, No. 868 (1946), from the Penrose Fund and the General Research Council of the Oregon State System of Higher Education for assistance in support of this work.



MAP 1. Sketch map of Northeastern Oregon showing the relation of the lower Snake and Columbia Rivers to the Great Basin. Cross hachure indicates area of terrace reconnaissance.

Such passage was feasible probably about 15,000 years ago.[2]

The second possible route that could have been followed was that of the Yukon-Mackenzie-High Plains. This would have brought the migrants to the easy pass of the Rocky Mountains into the Snake River drainage. By following this river they would have been in a position to branch off by a number of routes directly into the Great Basin or even to have passed on down to the Columbia and thence move southward into the Basin. In the latter case their problems would have been the same as for those coming by an intermontane route (map 1).

It is this theory of the Snake River passage toward which Sayles and Antevs (1941) lean. They would bring the early migrants into the Great Basin from the Snake River by means of its southern tributaries. This theory, however, on the basis of present information, seems to fail to meet the problem raised by the cultural differences between the High Plains and the Great Basin and by the close contemporaneity of the early occupation of the two areas. It is difficult to see how there could have been contact in Canada and the Northern High Plains between the two different types of cultures we have mentioned above without some mutual exchange of traits. It is true that almost no work has been done in the crucial area of the Snake River, so lack of information does not necessarily at present invalidate the theory.

The third possible route would have been by passage into the southern limits of the Great Basin after negotiating the High Plains route to the southern limits of the Rocky Mountains. There would then have followed a northward movement by which the Great Basin was populated.

The field work reported in this paper was concerned with one aspect of this problem, namely, the possibility of a southward movement of the Paleo-Indian from the Columbia River into the Great Basin by the way of the John Day River drainage. Our problem started with the assumption that the early Indians were on the Columbia but does not specifically involve either of the first

[2] Ernst Antevs in personal communications to the author.

two theories mentioned above. In historic times there were well known Indian trails connecting the upper John Day Valley with the Columbia directly toward the north and indirectly westward via the Deschutes River. A heavily used trail connected the John Day region with the Great Basin via the South Fork of the John Day to the Silvies River which flows southward into the Basin. At an earlier time the Silvies probably discharged into Harney-Malheur Lake. This route would have led any migrants following it directly into the Great Basin.

A University of Oregon reconnaissance party for the Oregon State Museum of Anthropology at the University, under the direction of Alex D. Krieger, carried out a survey in north central Oregon in June 1938 and the northern John Day area was included in this reconnaissance. The party located Butte Creek Cave about ten miles west of Fossil. They dug a test pit and found a basketry plaque similar to those found in the caves in south central Oregon in the Northern Great Basin by University of Oregon field parties. The similarity of this basket to those of the cave ware of south central Oregon and its dissimilarity to the known basketry of the area in which it was found immediately suggested a southerly affiliation but, of course, there was no evidence whether it was earlier than the cave materials or later. If earlier, it would mean priority and thus movement from the north to the south. If later than the cave ware, it might indicate a population which had been in the area before the Great Basin occupation occurred and which left behind people who continued to live in the area. However, it is obvious that the basketry could have implications for answering the question of the direction of migration of the Paleo-Indian in the area under discussion.

The distribution of L-shaped scapula awls (Cressman, 1942) from Washington to northern Arizona and the relative periods at which they were used in the different areas strongly suggested a southward movement of population from the Columbia or at least a diffusion of traits in that direction.

The theoretical possibilities of the southward movement and the two strongly suggestive pieces of evidence discussed immediately above indicated the John Day River area as an important area for examination to secure evidence bearing on the possibility of southward movement from the Columbia River.

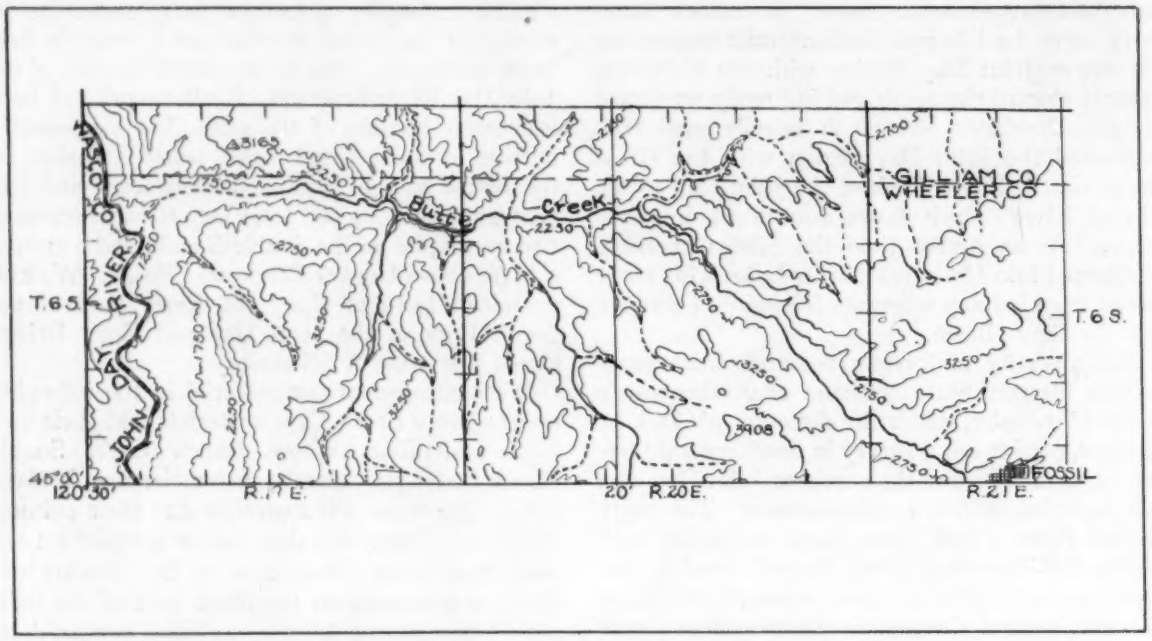
To attempt to secure evidence on this question, a small University of Oregon party under the direction of the writer carried out a month's field work in August, 1946, in the middle section of the John Day River drainage. Earlier work had been impossible because of the war. We excavated a portion of Butte Creek Cave, tested a shelter on the Kittoe Ranch, examined Pleistocene and Recent terraces along the John Day River, excavated two house pits on the Snable Ranch, and a cremation pit above Hoover Creek near Fossil. We also examined terraces along the lower part of the South Fork of the John Day and along Bridge Creek just west of Mitchell.

We shall discuss our material in the following order: Butte Creek Cave materials and their cultural affiliations, Kittoe Ranch Cave, Snable Ranch house pits, terrace reconnaissance, Hoover Creek Cremation Pit materials and their cultural relations. There will then follow a report on the skull from Butte Creek Cave by Dr. Stewart and finally a statement on the blood type of the individual represented by the skeleton, prepared by Dr. Laughlin. The summary of the paper will appear at the end of the section on cultural materials.

BUTTE CREEK CAVE

Butte Creek Cave (fig. 1; map 2), really a large overhang which apparently was formed by erosion before the canyon floor had been cut down, lies about ten miles west of Fossil which is approximately fifty miles south of the Columbia River along the John Day Highway, U. S. 28. The cave is in either the western part of Section 4, or the eastern part of Section 5, T6S, R20E, Willamette

FIG. 1. Butte Creek Cave. Burial was against wall, rear center. The two holes are the places from which Philbrick reported his specimens were taken.



MAP 2. Sketch of section from map 1 to show location of Butte Creek Cave and topography of adjacent area. T.6S. R.19–20–21E, Willamette Meridian. Adapted from USGS Oregon Condon Quadrangle. Contour Interval 500 ft.

Meridian, Condon Sheet, U. S. Geol. Survey, Ed. 1916. A deep fairly narrow canyon is cut through a series of lava flows capped by a final flow called brick-bat lava, from the fragmentary nature of the lava which erodes rather easily along the joint surfaces. A small stream, Butte Creek (fig. 2), flows at the bottom of the canyon, draining into the John Day River. The area is in the southern part of the Columbia Plateau (fig. 3) and a few miles to the south the physiography changes markedly to be dominated by the volcanic Miocene beds of the John Day formation.

The cave floor had been extensively "pot-hunted" and systematic excavation was useless. However, the place from which the basketry had been taken was undisturbed. We excavated this area as well as carried out tests in one undisturbed end and in the holes left by the previous diggers. Results, outside the place from which the basket had been taken, were negative.

FIG. 2. Looking south, down draw from Cave to Butte Creek.

FIG. 3. View across Butte Creek Canyon, south, from just outside of the cave to show the rounded Columbia Plateau topography.

FIG. 4. Portion of the Burial showing the cape or blanket in position.

FIG. 5. Burial with the cape removed showing the dog skeleton.

Our digging in the place from which the basket had been removed in 1938 soon exposed a skeleton and it was clear that the basket had been part of the burial complex. The corpse was buried on its back, probably in the extended position (fig. 4). Fire, which had later started in the dry grass in the cave, had burned into the grave and ignited the grass lining. The legs and some minor portions of the pelvis were completely burned, but there was no evidence that this was an attempted cremation.

A dog had been buried with the corpse on the left side of the pelvis (fig. 5). The fore-part of the skeleton of the dog was badly burned and all of the teeth were lacking. The corpse had been covered over the left shoulder by a cape or small blanket made of twined rabbit skin strips. At-

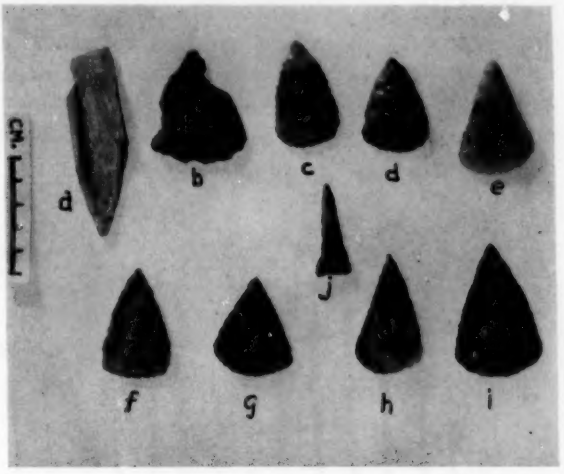


FIG. 6. Stone and bone implements. *a*, *c–i*, from Butte Creek Cave. *b* and *j* are from Snable House pits.

FIG. 7. Blanket of rabbit skin, wolverine fur, feather wrapped string, and tip of coyote tail before analysis.

tached to the cape was another piece of costume made from wolverine fur and attached to this was the tip of a coyote's tail. Feather wrapped three-strand cord was pressed into the mass of blanket and fur. The tip of a stone projectile point, a yellowish chert, had pierced from below the spine of the left scapula and the wound had not healed as shown by the splintered bone. At a shallow depth under the skeleton were found seven triangular shaped scrapers. Possibly they did not belong to the burial complex. A portion of hair was attached to the scalp, a part of which still adhered to the cranium.

No animal bones, aside from those of the dog, were found which could be used for identification and chronological purposes, nor did the situation of the grave in the cave make possible any correlation with any known geologic feature which might be used for chronological reference.

John Philbrick, government trapper in Fossil,

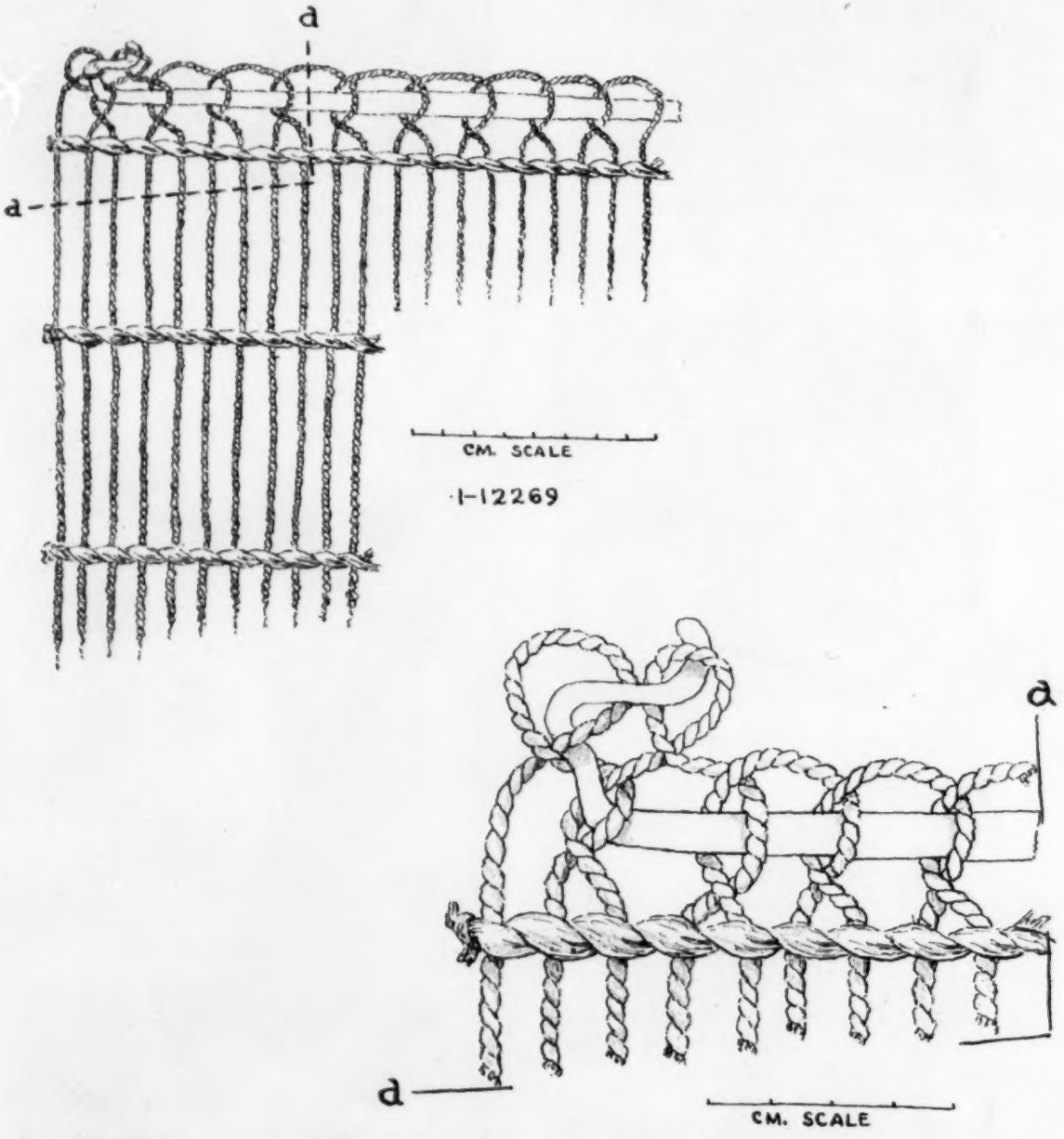


FIG. 8. Structure of rabbit skin blanket; (*a*) section of selvage enlarged to show details.

had some materials which he had, according to his statement, taken from this cave. His statement is probably to be relied on in view of the general similarity of the basketry fragments to specimens found in the debris of the grave. There is no possibility of placing them in a chronological reference to the burial. Philbrick's artifacts consist of fragments of Catlow twined basketry, fragments of matting, three stone club heads (?), a digging stick handle, four horn spoons, a digging stick, a bone bead, and a wooden needle for sewing mats. A fragment of a sewed mat was in his collection.

ARTIFACTS

STONE AND BONE (FIG. 6)

Seven triangular shaped gray chert scrapers of excellent workmanship and with convex bits were under the skeleton, but separated from it by a thin layer of dirt. They may be a part of the burial complex although slightly below the skeleton. In a dry cave burial of this sort, dirt is bound to work around the specimens as flesh decays. One bone gouge was found above the burial, but is probably not associated with it.

RABBIT SKIN BLANKET (FIG. 7)

The warp consists of two-strand strips of rabbit skin each strand of which is twisted clockwise and then both strands are twisted together in the same direction. Each warp without the hair is about 4 mm. in diameter.

FIG. 9. Method of attaching feathers on feather wrapped cord.

FIG. 10. Rabbit skin blanket after the other materials were removed.

The weft is of deer or antelope skin and the pitch of the twining is down to the right. Each of the component weft elements is about 5 mm. wide. The hair was probably removed before twining. The distance between weft rows is 36 mm. Each weft element seems to be a separate piece, that is it is not continuous running back and forth, but because of the fragmentary nature of the piece we cannot be certain. A small string tied with a plain overhand knot occurs in each loop of the weft at its starting point outside the first warp element on the left (fig. 8*a*). The function of the knot is not known but perhaps it served to tie some supporting string while the twining was being done. It also might have been used to bind the feather wrapped string as a border.

FEATHER WRAPPED STRING (FIGS. 9, 10)

Four pieces of feather wrapped string totalling approximately 20 cm. in length were a part of the blanket. Scavengers had eaten most of the barbs and even quills. One piece has a loop about 4 cm. long which might have been a slip noose but the condition is so fragmentary that it is impossible to be certain. However, it is clear that the string

was a part of the blanket possibly serving as a decorative border.

The foundation of the string is a three-strand bast twisted clockwise. Each of the three strands in turn consists of a fine two-strand cord twisted clockwise. The ends of the quills were cut off then the stubs were laid over one strand, the protruding end bent back under the strand and over a second strand. The third element of the string seems to have been used as a surface element and not used for binding directly. It would have helped in the binding however, as a component part of the whole cord. The feathers are then wrapped spirally back around the whole cord so that the piling covers it entirely. When in use, the feathers must have given a continuous thick body with the projecting piling.

Dr. Alden H. Miller, the Museum of Vertebrate Zoology, University of California, examined the fragments of feathers but they were too badly destroyed to permit identification. Dr. Miller stated that they could be breast feathers of any of a number of large birds.

FIG. 11. Fragments of mat, basketry, and string. Top row: two fragments of same mat. Center row, left is open work twining; other fragments in center row are Catlow Twine. Bottom row, string and piece of mat. All except bottom row are charred.

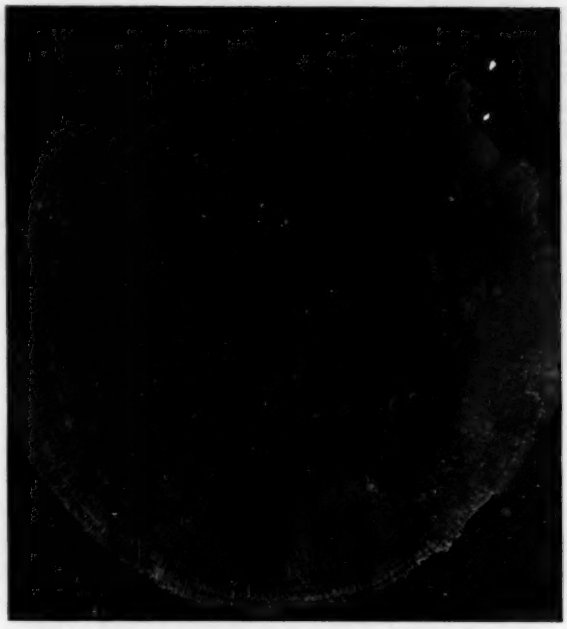


FIG. 12. Large twined tray or plaque from over burial.

BASKETRY AND CORDAGE (FIGS. 11, 12)

Two types of basketry are represented from the cave: (1) typical Catlow Twine and (2) an open twined ware. The Catlow Twine consists of one nearly complete plaque or sifter basket found by the 1938 party and several fragments of one or more baskets. The plaque (*cf.* Cressman, 1942; 42; 1943) which is 56 mm. in diameter was used as a cover for the corpse. Scorches on the basket suggest it was used as a parching tray but they may be due to other causes. The outer surface of the starting area is worn off so the method of starting cannot be determined.

The second variety is an open twined specimen, made from a fine bast string, and with the pitch of the weft stitch down to the right. This piece came from below the burial and is badly charred. This piece of basketry is consistent in type with specimens from the southern Oregon caves.

Two small pieces of cord, one with a clockwise and the other a counterclockwise twist, were found.

PHILBRICK SPECIMENS

A series of miscellaneous artifacts (fig. 13) from his collection will be described first. An

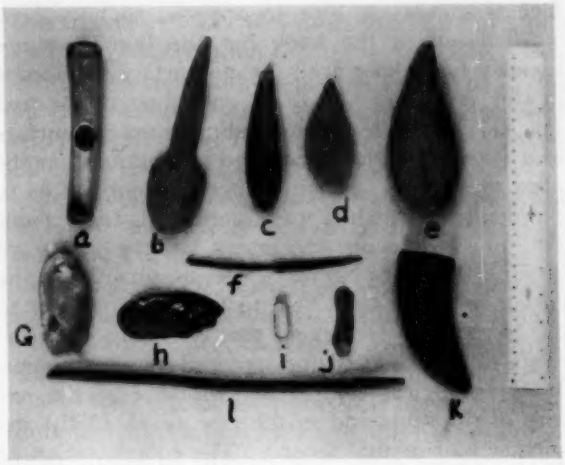


FIG. 13. Miscellaneous utensils in Philbrick collection. *a*, digging stick handle; *b–e*, horn spoons; *f*, wooden needle for sewing mats; *g–h*, unworked mussel shells; *i*, stone bead; *j*, spall scraper; *k*, horn cup (?); *l*, digging stick.

antler digging stick handle or weight, socketed to hold the end of the digging stick and enable the user to exert pressure on it, is one of the most interesting specimens since it has wide affiliations in the Pacific Northwest. A finely made flat needle of hardwood stands out for the quality of the workmanship. A simple digging stick completes the wooden articles. There are four horn spoons, one of which has a definitely shaped handle. Another horn artifact may have been a cup. Two unworked mussel shells, a bone bead, and a spall scraper complete this series.

BASKETRY, MATTING, AND STRING (FIGS. 14, 15, 16)

There is a piece of Catlow Twine. A decoration of a diamond pattern spiralling up the basket toward the right is put on with a combination of plain and wrapped twine overlay. The wrapped twining appears at the beginning and ending of each diamond. A black overlay material is used, probably a fine grass but I had no opportunity to have the material identified.

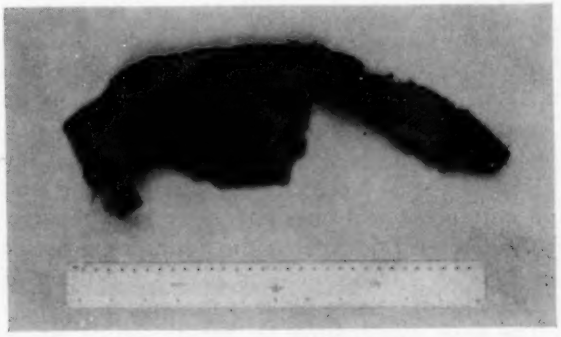


FIG. 14. Catlow Twine basket fragment from Philbrick Collection.

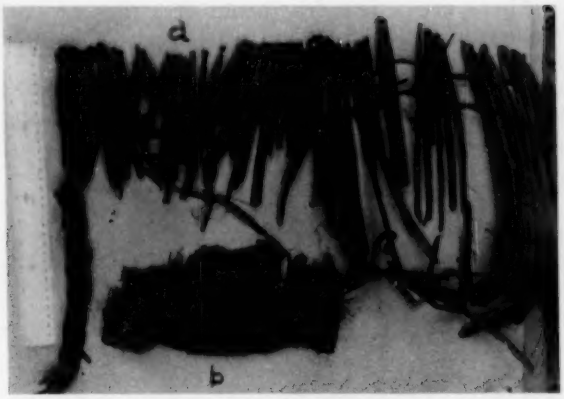


FIG. 15. Mat fragments from Philbrick Collection. a, sewed tule; b, twined grassbundles.

FIG. 16. String and mat fragments from Philbrick Collection.

There are two pieces of matting, each typical of a different kind. One is a sewed tule mat, the kind usually used as a house cover. It is finished with a three-strand braid, each strand of which is made up of two tule stalks. The top weft row is twined and catches the braid selvage in the twining. The balance of the piece is sewed and the thread passes through the selvage. The sewing thread is a two-strand piece with a counter clockwise twist.

The other fragments are from two grass mats with a twined weft, down to the right. One has a braided selvage at the top made by braiding the warp elements together into a three-strand braid.

There are a few fragments of string or cord made from grasses and constructed with a counterclockwise twist.

FIG. 17. Small stone hammers or club heads from Philbrick Collection.

STONE OBJECTS (FIG. 17)

There are three stone objects, the use of which is uncertain. They are shaped like mauls or stone club heads. Each is partially grooved for hafting and the black one has a small cup shaped depression which might have been used for hafting. Or the depression might be used for a knot on the rope to which it was fastened as was frequently the case with net sinkers in the Pacific Northwest.

CULTURAL RELATIONS

SCRAPERS

The Butte Creek Cave artifacts from the burial are difficult to place in any cultural context for this region. The triangular shaped scrapers, which were just below the burial and may or may not be associated with it, are unusual. Cressman (1942: fig. 40, and table 23) illustrates the type, but those have a straight bit, and the Butte Creek scrapers have a convex edge. Strong *et al.* (1930: pl. 12–19, *f–i*), show triangular "points." I believe these latter to be end scrapers on the basis of experience in classifying numerous similar specimens in early collections for the Dalles region of the Columbia River. Microscopic examination showed pitch on the sides of the contracted portion, obviously evidence of hafting. Strong's specimens are straight across the bit whereas ours are curved with one exception and that suggests Strong's specimen L, pl. 12. Krieger (1928) does not list the type from Wahluke. Collier *et al.* (1942) illustrate two specimens strikingly like ours, pl. I*b*, *d*, and pl. II, P. The last two specimens are indicated as surface finds. Some surface specimens at Site 45 are said to have been washed out of the bank but the text does not identify more specifically than "surface the source of this scraper." Pl. I*d* is, according to the text (p. 35), from a cache extending from the surface to a depth of eight inches and is identified on the plate as surface. Pl. I*b* cannot be identified as to the depth at which it was found. Pl. I*d* is about the same size as our specimens, but the others are larger. These Columbia River specimens are the closest we have been able to find typologically to our scrapers. However, the Washington specimens cannot be used for stratigraphic description or chronological comparisons because of the nature of the finds and in the case of pl. I*b* the inadequacy of textual record. The concentration of this single type of scraper in Butte Creek Cave and its possible use as grave furniture must have some symbolic meaning, but what it may be is unknown. At any rate, there is no frame of reference into which they may be placed as far as this function goes. All we can say is that similar forms appear to the north along the Columbia but their chronological significance is unknown. The type of scraper on the basis of present information seems to belong to the Columbia Plateau.

CLOTHING

The blanket or cape consisted of four pieces: rabbit skin, wolverine, feather wrapped string, and coyote tail.

The piece of wolverine skin has had all the hide eaten by scavengers so it is impossible to determine how it was fastened to the rabbit skin section, but there can be no reasonable doubt that it was attached originally as a part of the larger garment. Bailey (1936) writes: "Wolverines once occupied the boreal zone across the northern part of the continent and southward in the mountains to Colorado and California. At present they are rare in the United States, but probably are not yet extinct in the Cascades and Sierra Nevadas. Adolph Aschoff reported them in the Mount Hood section in 1896, and George Moody caught one in the Upper McKenzie Valley west of Three Sisters Peaks, Oregon, in 1912" (pp. 300–301). The habitat of the wolverine is the Canadian life zone and only occasional ones have ever been reported in the United States from the appropriate mountain habitat. This condition makes the presence of this hide the more difficult to explain.

The easy explanation is that this hide comes from an animal living at a time when the climate was colder than now with the habitat more wide-

spread and at lower elevations resulting in a more extended distribution of these animals. However, this explanation is probably too easy. Such an explanation, if sound, might put the time in the early part of the Late Post-Pluvial or between 2000 B.C. and the beginning of our era.

It is difficult to explain the hide as a trade piece in fairly recent times or even as that of an animal killed by a hunter in view of their scarcity and limited range. The nearest locality in which they might have been found was seventy-five miles as the crow flies over extremely rugged country to the west, either Mount Hood or Mount Jefferson. Ray (1942) does not record the use of wolverine hide for the Plateau although the use of skins of other types of animals is listed. Stewart (1941, 1942) and Steward (1941) do not report this hide for the Basin, nor is it recorded in any of the extensive material, both ethnographic and archaeological, for the more southerly regions. Either it must represent a lucky and unusual kill, or it might be representative of an earlier and colder climate as suggested above.

RABBIT SKIN CAPE

The rabbit skin cape or blanket is used throughout the entire Plateau and Basin and into the Southwest. Unfortunately the technical details of selvage, etc., needed for comparison are not given in the ethnographic accounts and only rarely in archaeological reports. There are good reasons for this because the ethnographer usually does not undertake to analyze a specimen, even if one is available, and the archaeologist has to depend on chance preservation of the specimen to have anything to analyze. The outstanding exception is that of Guernsey and Kidder (1921) and Guernsey (1931). Guernsey and Kidder (p. 65, fig. 11*a*) show a Basket Maker selvage. The weft is yucca cord. The illustration does not show whether the initial weft row was fastened but from the drawing it would appear to be held in place only by the tension of the twining. The weft cord would seem to have been doubled over the outside warp strand on the left and immediately twined by a down-to-the-right pitch. At the end of the row the two strands are twisted in a counterclockwise direction for a sufficient distance to place the next weft row and then twined again with a down-to-the-right pitch. However, since the direction of movement is from right to left, the manipulation of the twining motions would have had to be reversed from the preceding row.

Guernsey (1931: pl. 54*a*) illustrates a complicated selvage from a Pueblo I site, Burial 2, Cave 1, Segi. A finishing cord runs around the entire edge. The first weft row is held in place by being caught through the first fold just outside the weft and the loop is then knotted over the finishing string. The first weft row stands singly as though to give rigidity to the end of the blanket and to hold the warps apart. Succeeding weft rows are fastened in pairs by interchange of single twining elements. This blanket is more advanced technically than the Basket Maker type previously described.

Guernsey (1931: pl. 54*c*) shows another type from the burial of a child in the Segi, Burial 3, Cave 1. The caption refers to the specimen as a feather-cloth blanket but in the text (p. 193) this diagram is used to illustrate fur string blankets as well. This specimen is of particular interest. We have assumed the selvage on the Butte Creek Cave specimen to be the top and starting point. Guernsey's specimen shows the lower part to be finished quite similarly to the Butte Creek Cave piece. Instead of the warps running parallel they are placed so that the ends are spread out into widened loops. In that way the looped sections overlap to form secondary loops. The loop will then have one side under the loop on its right and the other over the left adjacent one. As a stabilizing element two (untwisted) strings are run from side to side through the loops formed by the overlapping and then are tied on the left side. The disposition on the right side is not shown but perhaps the two strings simply are folded back over the outer warp. The other end is simply finished with the warps turned back at the proper distance to keep them parallel. The first weft is knotted to hold it in place. The weft series are paired much as those in the other Pueblo I specimen. I think it is open to question that the blanket was made in the position Guernsey believes. The obvious function of the selvage assigned by Guernsey to the bottom is to give greater firmness to the relative position of the warps, a requirement of more importance at the start than the end. Hough (1914: 72, fig. 149) figures a feather jacket from Tularosa Cave. The crossing of the wefts in this is like the Butte Creek Cave method and occurs at the top of the jacket.

The Butte Creek Cave specimens (fig. 8) shows almost exactly the same kind of selvage as Guernsey's second specimen, whether it is top or bottom. We do not know how the other end and side were finished because they are lacking. Our warps are formed into secondary loops, in the

same manner as the Burial 3, Cave 1 blanket. Instead, however, of two untwisted weft stabilizing elements a flat strip of leather is used in ours for the same purpose. This strip is knotted into the first two pairs of wefts as shown in the drawing (fig. 8). When this knot was pulled tight it held very effectively, as I found out in analyzing the specimen, and it made only a slight bulge in the fabric which was well concealed by the rabbit fur. The weft strands are narrow strings of hide, possibly deerskin. Each weft, as far as can be determined, is a separate pair of strings. However, each element has a plain overhand knot tied in it at the point of the fold. This could conceivably have been used to tie some binding cord or border decoration to the cape. It is suggested that the feather wrapped string to be discussed next might have been attached to these knots by an intermediate fine string which has disappeared. The knots do not seem to enclose anything as would be expected if the feather wrapped string had been directly enclosed.

The Basket Maker and Pueblo I specimens use yucca cord for the weft. It is puzzling in view of the use of sagebrush bark cord as shown by the other Butte Creek Cave remains that the weft in the cape should be hide. In the other cave remains from Oregon hide is only sparingly used. Roaring Springs Cave (Cressman, 1942: fig. 91K) produced a child's sandal woven of rabbit skin strips and grass rope.

FEATHER WRAPPED CORD

This is the only piece of feather wrapped cord reported north of the Anasazi area in the intermontane region. Blankets, or capes made of strips of bird skins with the feathers attached are reported ethnographically for the intermontane region and archaeologically from Lovelock cave.

Feather wrapped string is reported for the Basket Maker horizon (Kidder and Guernsey, 1919: 118, 174). Most of the Basket Maker feather string specimens found by Kidder and Guernsey were made from strips of bird skins wrapped on the yucca cord foundation. The authors say in a footnote on page 174: "More rarely with tufts of feathers caught between the twists of the cord after the manner of 'fluff string.'" Guernsey (1931: 93) definitely assigns the first true feather wrapped string to the Pueblo I horizon. By Pueblo III times, feather wrapped string blankets are in common use. Kidder and Guernsey (1919: 174) state that fully nine-tenths of the blankets, "Cliff-Dwellers," are made of feather wrapped string with the large wing and tail feathers of the turkey commonly used.

Hough (1914: 71, fig. 148) shows the method of fastening the feathers in and around the cord in the Tularosa region. The technique is essentially the same as that described by Guernsey for the region further north and similar to that used in the Butte Creek Cave specimen except in the latter a three-strand cord is used.

In the opinion of the writer there is a cultural affinity between the Butte Creek burial and the Anasazi area. The techniques used in the blanket selvage and in the feather wrapping in the Pueblo I are so similar to the Butte Creek Cave specimen that it is difficult not to see cultural connections. However, there is a vast area in between completely lacking any similar specimens as far as present knowledge goes. What the relative period of the Butte Creek Cave specimens may be with reference to the Anasazi is unknown. The Oregon situation is complicated by the wolverine hide used in the garment, but it must be admitted that it may be there as a result of a chance kill and thus be without significance. It also must be admitted as a possibility that there may be another explanation involving a different climate and thus an earlier period.

The overall impression of the burial (except for the seven scrapers, which might have been a cache made before the burial, and their appearance under the body would provide a purely fortuitous connection with it) is strongly that of the typical Basket Maker II complex. The body probably was in the extended position, not a B.M. II trait, although there may have been some flexion but the burning of the legs concealed this condition. We have the cave burial, the dog buried with the body, the cape with the details described, and a large basketry plaque covering the burial. Occasionally in the Plateau a dog was buried with its master, but this complex is different from the customary Plateau-Basin practices.

Cressman (1942) has advanced an argument for the priority of Northern Basin culture over the Basket Maker. The Butte Creek burial certainly seems to indicate Oregon-Southwest connections in Pueblo I times but there is no way at present to indicate relative antecedence.

A strong piece of evidence in favor of Oregon-Southwest cultural connections, comes from other sources and is introduced here to reinforce our arguments for contact, and if our interpretation is correct, then the direction of diffusion even at that late period involved was southward. Jeancon

(1923: pl. 59, A) illustrates a "Koshare Fetish from the Chama." Kidder (1932: fig. 75) shows a "problematical object of lava." This specimen comes from a cache of religious paraphernalia characteristic of Glaze V, i.e., Pueblo IV. The specimen according to Kidder has "one edge of the oval base curved upward as if by wear." The material is scoriaceous basalt and a good material for grinding purposes. Kidder refers to the "almost identical object" found by Jeancon.

Barrett (1910: pls. 12,–1, 21,–2) illustrates the two handled or two-horned manos or mullers used by the Klamath and Modoc especially for grinding wocas (water lily) seeds. Kroeber (1925: 324, fig. 27) figures and describes these instruments. He writes (p. 325): "It is likely that the two-horned mulling stone, which seems to be *restricted to the Modoc and Klamath* [italics ours], is to be traced directly to the *wokas* industry." This type of instrument is so distinctive of the Klamath-Modoc, and especially the former, that it has almost the identifying qualities of a trade mark. The similarities between the two southwestern specimens, unique in that area, and the Klamath-Modoc type are so great that no other inference is justified than that they are derived from the same source. They are a specialized utility article among the Klamath-Modoc; in the Chama and at Pecos both were used for ceremonial purposes. Their strangeness in the Rio Grande area and their two horns would combine to give them a character which would readily fit them into the Koshare complex. If our arguments here are sound and we cannot find any to the contrary of equal conviction, then we have distribution by some method into the Southwest, as far as the Rio Grande, from the Oregon region and by Pueblo IV. While this proves nothing about the relations between Butte Creek Cave and the Southwest, it illustrates another facet of the problems of Northern Basin and Southwestern relations and defines problems for future archaeological research in the intervening areas.

The Philbrick specimens are typical of the Northern Basin-Plateau basic culture. There is nothing distinctive about them to put them in any specific horizon. The digging stick (handle) is certainly found in the Fraser and Columbia River area and south in the Willamette Valley in recent times. How far back its use goes is not known. Cressman and Perry (1938) reported a horn spoon from an ice cave near Bend, Oregon, with a dendrochronology date on a cut log in the cave of 1370. However, there is no way of relating the spoon to the log in time. It may be earlier, contemporaneous, or later. All the specimens in this series may be historical or considerably earlier. The only arguments against real recency are that the specimens represent cave habitation and that is not recent in the area; and there is the use of sagebrush and grass for cordage and complete lack of leather, again a basic early Plateau complex. It is probably safe to infer that these specimens, if from this cave, are later than and entirely independent of the burial for it is unlikely that the cave would have been used at the same time as a burial place and for habitation. The burial complex gives the impression of being older than the other materials and it is probable that if the cave were used for habitation the people using it were quite unaware of the burial back against the rear wall.

Anticipating the reports by Stewart and Laughlin, the craniological evidence as reported by Stewart indicates the individual whose skeleton was excavated from Butte Creek Cave belongs to a physical type found in the Pacific Northwest. Laughlin's report on the blood group type strongly suggests the same affiliation. However, since but one individual is represented and the blood group test for type B is inconclusive, this evidence has to be accepted at present as tentative.

The physical characteristics and the culture traits do not support each other in the matter of areal relationships. This discrepancy is probably not to be wondered at in an area so well known for population movements and cultural shifts as that adjacent to the Middle Columbia River. The most that can be said in summary is that the conflicting evidence of relationships tends to sharpen

FIG. 18. Cave on Kittoe Ranch, 3 miles south of Clarno on east side of John Day River.

the character of cultural and population problems in the Pacific Northwest.

THE KITTOE RANCH (FIG. 18)

A small shelter on the east side of the John Day River about 3 miles south of Clarno was excavated with negative results. The cave contains some pictographs in red paint and some 30 meters across the draw to the south is a striking assemblage of pictographs. While the cave showed heavy deposits of ash, dust, weathered rock fragments, and flakes of obsidian, no complete artifacts of any kind were found.

THE SNABLE RANCH HOUSE PITS (FIGS. 19, 20)

Two house pits were excavated on the Snable Ranch but produced little or no material of value.

FIG. 19. Site of Snable Ranch house pits. Cover is sage brush. Upper right corner back of trees appears a typical John Day tuff and mud formation which weathers easily.

FIG. 20. Floor of house pit on Snable Ranch showing large stones and lack of definition of house pit walls and floor.

The pits had been dug into the soft beds of tuff washed down from the cliffs (fig. 19). The later fill in the pits had so much the same consistency of the original deposit that we could not determine the pit walls with any certainty. No timbers or post holes were found. One pit (fig. 20) contained three large flat stones which might have been used as post supports in the soft tuff. However, there was no evidence of posts although one pit contained several charred fragments of poles two to three inches in diameter. The smooth surface of the stones suggests their use as metates. The shelters might have been temporary summer houses or the dome shaped house of the Basin type with the wooden framework covered by mats. A more permanent type of house than the summer lodge is suggested by the large stones which were obviously carried in. Small pliable saplings of the type used in the Basin type of house, unless protected by a covering of dirt, could easily have rotted (along with any mats). There are other house pits in the area and the excavation of these might throw some light on the nature of the houses in the region. Obviously these two do not. The time involved in contacting the owner of the land on which the other pits are located did not permit us to undertake the work that season.

A poorly made scraper (fig. 6) perhaps an effort to make a triangular type and a drill (fig. 6) came from these pits.

TERRACE RECONNAISSANCE

The reconnaissance along the terraces produced negative results. A bilaterally flat, oval mano, like those found throughout the Basin region and the Plateau in both prehistoric and historic times, was found on a terrace west of Mitchell, but it was impossible to tell whether this was a quite recent artifact or one of some antiquity. The weathering of the rock suggests antiquity but the antiquity may represent the time before it became a mano. There was no way of tying it in with the period of terrace building. Any reconnaissance along the John Day terraces is not likely to be fruitful except by mere chance. The cliffs rising above these terraces, in some cases a thousand feet, are composed of Miocene muds, sand, and tuffs. As they erode this material is washed down over the terraces. The terraces are also made up of reworked Miocene materials so that there is a constant process of sheet erosion covering the terraces. Cuts through the terrace by streams very seldom give any clear exposure of the terrace banks because of the erosion of material from above. We

do not have a situation similar to that provided by the arroyo cutting of the Southwest.

HOOVER CREEK CREMATION PIT (FIG. 21)

About one-quarter mile east of the highway and a mile north of Fossil, Section 15, T6S, R21E, Willamette Meridian, where the road makes a hair-pin turn climbing out of the valley, there is an extensive talus slope. There was a large pit in the talus in which many fragments of calcined bones were found (fig. 21). There were a piece of hammered copper, probably a pendant, two fragments of small, carved, stone pieces, and several fragments of pipes. There were also dentalium beads. These fragments were found by excavating the pit which had been dug by throwing the stones from the center to form a wall around it. The cremation fires had been sufficiently hot to fuse the basalt, in some cases permitting it to flow around fragments of bone and a pestle (fig. 22). Informants have reported this type of cremation from near Heppner, about sixty miles northeast of Fossil, and just west of Antelope, approximately fifty miles west of Fossil. I have seen the same kind of fused rock containing bones from the Heppner locality and specimens from Antelope similar to those found in the Hoover Creek cremation. It is probable that we are dealing in these cremations with a southerly extension of that complex reported for the Columbia; however, the problem is one that needs extensive study. A point of great interest is the method of firing in these cremations to produce sufficient heat to fuse the basalt (*cf.* Strong *et al.*, 1930: 25).

FIG. 21. Hoover Creek Cremation Pit in talus slope north of Fossil. One man is crouching in pit and one standing for scale. Man on right is standing on south wall while one on left is just outside the wall.

FIG. 22. Chunk of fused basalt from Hoover Creek Cremation Pit. Note three fragments of pestle with one end attached to small piece of fused basalt and the other embedded in the large piece, while the center portion is free. The bone in the large piece is a human radius. *Cf.* Strong *et al.*, 1930: 25.

Among the artifacts from the cremation pit (figs. 22, 23) is a pestle broken into three parts. One portion is fused into the basalt melted by the cremation fire and another has the cooled basalt adhering to the end of the pestle around which it flowed in a molten state. A human radius is also enclosed in the fused basalt.

Three dentalium shell beads, two fragments of tubular pipes made from micaceous sandstone, a crescent shaped copper pendant, and two pieces of carved stone were also found in the debris of the pit. The two pieces of carved stone may be fragments of ornaments or perhaps incompleted pipes because there is no hole drilled through either.

CULTURAL RELATIONS—HOOVER CREEK CREMATION PIT

This site is definitely within the area of the Columbia River cremation practices as described by Strong *et al.* (1930: 48–50). The fusing of the basalt into a slag-like mass (fig. 22) is described for the cremation on Millers Island in the Columbia off the mouth of the Deschutes by Strong and his associates (p. 25). The artifacts found at the Hoover Creek Pit (fig. 23) tie this site in with northerly relations. The dentalium shell beads, the pipe fragments of micaceous sandstone

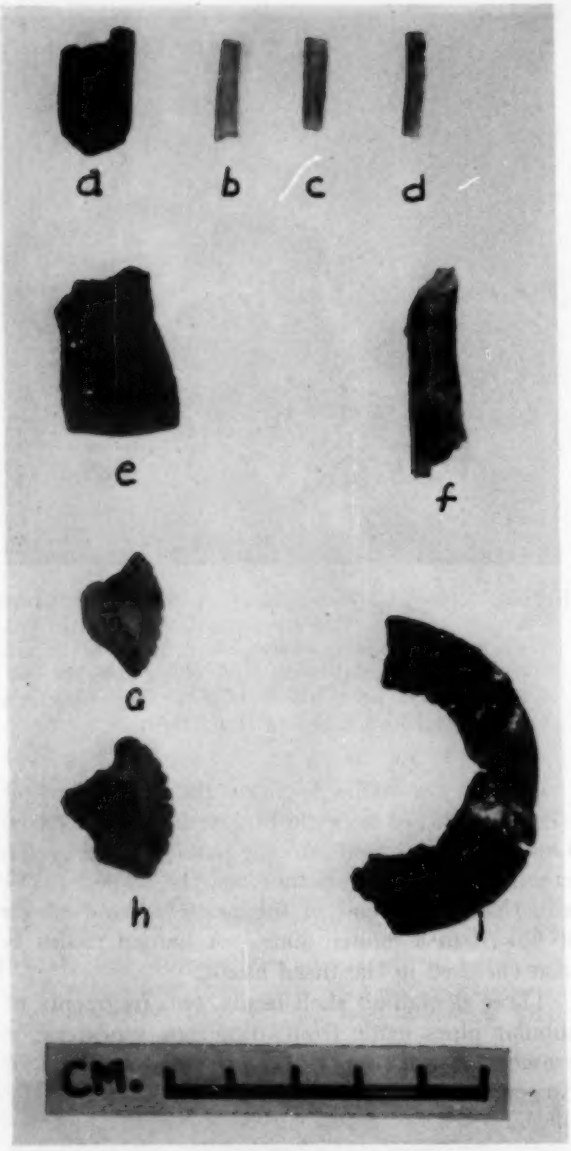


FIG. 23. Miscellaneous pieces from Hoover Creek Cremation Pit. *e*, piece of pipe; *b–d*, dentalium shell beads; *f*, carved bone; *g–h*, pendants (?); *i*, copper pendant.

and the copper pendant all point toward the Columbia. More important, however, than these is the fragment of bone carving in exactly the same style as represented in the Columbia River cremations (Steward, 1927). The small fragment shown in figure 23 has the concentric pattern illustrated by Steward (1927: pl. II). There were none of the more elaborate carvings characteristic of the Dalles-Deschutes region found at Hoover Creek. Their absence may mean the Hoover Creek cremations were earlier than the period of the more elaborate art forms along the Columbia or that the complex did not diffuse southward. I have seen fragments of stone "slave-killers" reported to have been found in cremations near Antelope, west of the John Day River and great numbers of pieces of carved bones come from these sites. However, the association may be fortuitous for I know of two such "slave-killers" having been found near Harney-Malheur Lake south of Burns, Oregon, and not associated with any cremation or burial.

The two small fragments numbered 75 (fig. 23) may have been pendants. They flare out with the rims notched. Both are drilled and the one next to the end has had the broken edges ground smooth. They do not seem to have been intended for pipes as shown by the drilling or perhaps it would be more nearly correct to say lack of it.

Strong *et al.* (1930: 49–50) suggest that in prehistoric times cremation was practiced from the Tsimshian via the interior of British Columbia throughout the Plateau to the Columbia River and that the lack of cremation in prehistoric times in the middle (Salish) area, was due to the introduction of burial by these people, and that since the Dalles-Deschutes cremations are in some cases post-Caucasian that the practice "persisted longer" at this point. Perhaps if the custom is northern in origin it not only lasted later at the Dalles-Deschutes region but reached there considerably later than the more northern areas.

The Hoover Creek pit is about on the boundary between the Tenino and Umatilla (Ray, 1942, map) and Ray reports cremation lacking for both groups (p. 218). Cremation was a custom among the Klamath (Spier, 1930) but they were a considerable distance to the south and west. Stewart (1941: 412) reports widespread cremation among Northern Paiute in Nevada, but not among the Oregon bands except the Quinn River group in the extreme southeastern part of the state. There is possibly a connection between the Paiute-Achomavi-Klamath-Modoc and/or California cremation custom, but this seems to be a complex separate from the northern one.

The copper pendant at the Hoover Creek site probably indicates a post-Caucasian site. On the other hand, the denial of cremation for the groups in the area after white contact (Ray, 1942) would argue that the practice dated from some antiquity. In that case it would represent the southern limits, as now known, of an early widespread cremation complex.

Since this manuscript was submitted the River Basin Survey under the direction of the Smithsonian Institution has excavated a site, 45BN3, on Berrian's Island in the Columbia River, about 5 miles upstream from the McNary Dam. This site is north and somewhat east of the Fossil locality. The site showed cremation, use of copper and iron, and pipes with a flange or flaring mouthpiece. The flange is usually notched transversely all around and in some cases has one or more holes drilled through it. These fragments from the Hoover Creek Cremation site are clearly fragments of pipes like those from Berrian's Island. Our fragment *g* is a half of a mouthpiece which was broken then abraded on the inner side to a flat surface and used for a pendant. This association of types of pipes, cremation, copper and iron, and other artifacts tie in this locality, the Berrian's Island site, and Site 21 of Strong *et al.* (pl. 24 l) from the Dalles-Deschutes and thus indicates that the Hoover Creek cremations are a southerly extension of the Columbia river culture.

On the other hand, if it is late, say post 1800, it might be considered a southward expansion of Sahaptin influence (Ray, 1939; Ray and others, 1938). This theory, however, is difficult to accept in view of the contrary evidence for the region offered by Ray in 1942. Perhaps the best theory is that it represents a pre-Sahaptin practice, a peripheral manifestation of the Dalles-Deschutes-British Columbia complex with strong affinities to the adjacent area, and that the Sahaptin occupation of the area later introduced burial customs as Strong *et al.* suggests the Salish did in Central Washington.

SUMMARY

One month's field work in the John Day region of North Central Oregon devoted to the problem of the direction of Paleo-Indian population movements in that area in connection with the Great Basin has shown at least two cultural horizons and possibly three in the area but failed to contribute useful evidence on the basic problem. The earliest occupation is represented by the Butte Creek Cave burial which seems to have its closest cultural affiliations with Pueblo 1 of the Southwest, although the evidence from the skeletal material suggests that the individual belonged to the population characteristic of the Pacific Northwest. While the cultural materials suggest affiliation with Pueblo 1, there is no way of determining the relative priority of the Butte Creek cave specimens and Pueblo 1. The Philbrick specimens represent a later second horizon and are characteristic of basic Plateau types. The Snable Ranch pit houses furnished no evidence that could be used. The third and latest horizon is represented by the Hoover Creek cremation site; it is more recent than the others and oriented toward the north with its closest relations with the Dalles-Deschutes area of the Columbia. It seems to be a peripheral manifestation of a widespread northern complex and is either proto-historic or early historic.

The reconnaissance of the terraces along the John Day River, the South Fork of the John Day, and Bridge Creek west of Mitchell gave negative results.

REPORT ON SKELETON FROM THE BUTTE CREEK CAVE, JOHN DAY RIVER REGION, OREGON

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The circumstances surrounding the recovery of this specimen are set forth in the report by Dr. Cressman.

Judging from photographs of the burial, not all of the skeletal parts recovered were submitted for examination. The following is a list of the parts upon which this report is based: skull and lower jaw with all teeth; left scapula, humerus, radius and ulna; distal third of right humerus (burned); right clavicle with manubrium attached by tissue; complete presacral spine mostly still united through the intervertebral discs (vertebral formula: 7C, 12T, 5L); pelvis, consisting of sacrum (5 segments) and damaged innominates (all burned except upper two segments of sacrum and parts of ilia).

This partial skeleton, which bears the catalog number 11–218, represents a young adult male. The pelvis, as well as the skull, gives clear indication of the male sex. The age, which was prob-

ably around 26–28 years, is indicated by the signs of concluding epiphyseal union. These signs appear at the proximal end of the clavicle, the iliac crest, the proximal end of the humerus and the distal ends of the radius and ulna. Epiphyseal union at these points is far advanced. The main sutures of the skull are still open, but premature closure has taken place in the posterior portion of

FIG. 24. Side, front, and top views of skull No. 11–218; oriented in the Frankfort position. Note asymmetry due to occipital flattening, slight deviation of nose to the left, and incomplete twinning (?) of the upper left lateral incisor. Original photographs half size. Photographs courtesy of the Smithsonian Institution.

the right squamous suture and in the parietomastoid suture.

Minor interest attaches to the teeth. The upper left lateral incisor has an extra element fused with it and probably represents a case of incomplete twinning. All four third molars are lacking congenitally. It is notable also that this individual probably had suffered a brief period of malnutrition (illness?) at about the age of four years. The evidence of this is seen in a narrow hypoplastic groove at the appropriate level on the enamel surfaces of the canines and upper incisors.

An abnormality of some cultural significance is the moderate lateral occipital flattening in the skull. Although the plane of flattening is not strictly vertical as in cradleboard deformity, neither is it typically lambdoid in type. The degree and asymmetry suggest that it was unintentional.

One other general characteristic remains to be mentioned, namely, size. For the computation of stature only the left humerus (maximum length, 301 mm.) and radius (maximum length, 237 mm.) are available. These two bones do not give the highest correlations with stature, and the available means of correlation are not entirely suited for Indians. Perhaps this accounts partly for the low estimate of stature obtained, namely, 5′3″.

The main reason for this report is to record an impression regarding the direction of affiliation of this skeleton. Obviously under the circumstances, this impression must be based mainly upon the skull (fig. 24). The skull yields the following basic measurements:

Length	(180)	mm.*
Breadth	(146)	*
Cranial index	(*81.1*)	*
Basion-bregma height	130	
Mean height index	*79.8*	
Facial length, total	123	
Facial length, upper	76	
Bizygomatic breadth	138	
Facial index, total	*89.1*	
Facial index, upper	*55.1*	
Diam. frontal minimum	97	
Basion-nasion height	102	mm.
Basion-alveolar point	100	
Nose length	56	
Nose breadth	23	
Nasal index	*41.1*	
Orbit length, right	38	
Orbit length, left	38	
Orbit breadth, right	37	
Orbit breadth, left	35	
Orbital index, mean	*94.7*	
Bigonial breadth	107	

* Measurement affected by deformation.

Of these measurements the following are of particular significance: basion-bregma height of 130 mm. (mean height index, 79.8); and the nasal diameters: length, 56; breadth 23 (index, 41.1). The relative lowness of the vault and narrowness of the nose appear to relate this skull to the Indians of the Northwest rather than to those of the Southwest. The Pueblos and Basket Makers of the Southwest are just the opposite in this respect; they are highheaded and have relatively broad

noses. A broad nose but not a high head also characterizes most Shoshoneans. On the other hand, I have seen many examples of narrow noses combined with low vaults among skulls from Oregon, Washington, and British Columbia. A good example from a Salish group is shown by Oetteking (1930: pl. 8). Although comparative material with documentation from Washington and Oregon is not available to me, I feel that there is no justification in regarding the type of the Butte Creek Cave skull as anything but a local development.

TESTS FOR PRESENCE OF BLOOD GROUP SUBSTANCE IN SKELETON 11-218

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At the invitation of Dr. L. S. Cressman, tests for the presence of blood group substance in skeleton 11-218 were performed. These tests were conducted, in conjunction with tests on Pre-Aleut skeletons, in a laboratory of the Department of Anthropology of the Peabody Museum. The sera were secured from Dr. W. C. Boyd, of the Boston University School of Medicine, who also provided advice on the conduction and interpretation of these tests.

A description of the technique employed in these tests may be found in Candela (1939). This is an absorption technique which involves the application of serum of known strength to a specimen of pulverized bone. After a period of incubation the supernatant serum is drawn off the specimen and tested against fresh red cells of the appropriate group. If the group substance, which determined the blood type, is present in the specimen of bone, the antibodies of the serum are wholly or partially absorbed and the strength of the serum is thereby reduced. The consequent failure to agglutinate red cells is presumptive evidence that agglutinins were removed from the serum and therefore that the group substance was present in the skeleton tested.

DESCRIPTION OF TESTS

Dr. Cressman submitted ribs, vertebrae, and some adherent tissues such as an intervertebral disc for the test. These bones were in excellent condition, and in all cases intact. The ribs were cracked in order that the cancellous tissue could be scraped out with a bone curet. When the tissue was pulverized in a mortar it gave off the characteristic odor of bone in which organic substances are present. To remove the cancellous tissue from the body of the vertebrae the cortical bone was first cut with a knife, removed, and then the cancellous tissue was scraped out with the curet. It might be noted that this method leaves the walls of the vertebrae intact and unimpaired for measurement.

SERA

Two kinds of sera were employed. The first was a high titer sera. This sera had been drawn from students who had received injections of A substance made from hog stomach. It should be noted that this kind of sera is altered in at least three ways: (1) the titer is increased; (2) it will no longer agglutinate A^1 preferentially, but will agglutinate A^2 the same as A^1; (3) more dilution is required to find the end point; this results in its protein content being lowered and it is known that the protein stabilizes the agglutinins. Consequently, in most cases such a high titer sera is not the best to use for this kind of test. In seven tests using a high titer sera with an end point of 1:800 the known A skeleton cleared all the agglutinins in seven cases. Specimen 11-218 cleared all agglutinins in two cases, cleared most in a third test and reduced it in others. By contrast, 11-218 cleared all agglutinins in all tests with low titer sera. The second serum employed was ordinary human sera drawn from students who had received no injections. This had a limiting titer of 1:64 or 1:128.

TITRATION

These sera were titered by testing successively doubled dilutions, e.g., 1:2, 1:4, 1:8, 1:16, 1:32, against a fresh 1 per cent cell suspension. If the 1:32 was the limiting titer, that is if it gave a $\pm$ reading whereas the next higher dilution showed no agglutination, this was then taken as the end point. Counting back three dilutions to 1:4 gave the dilution to be applied to the specimen. These dilutions were made in 13×100 mm. tubes. From each of these tubes 0.05 cc. was drawn, by

means of a tuberculin syringe, and placed in a 10 × 75 mm. blood grouping tube. To each of these dilutions was added 0.05 cc. of 1 per cent cell suspension. These were then placed in a rack and shaken at four minute intervals for thirty-two minutes. Readings of the degree of agglutination were made with the low power of a microscope. Cell suspensions were measured and washed, and the titration was performed before each test.

IMMERSION OF BONE SPECIMEN

The cancellous tissue was pulverized before each test in a mortar and 0.25 gram placed in a 13 × 100 mm. tube; 0.9 cc. of serum was added to this and then mixed with a glass rod and by rotating the tube. For each skeleton tested a separate tube was prepared for the anti-A serum and for the anti-B serum. Each tube was sealed with a cork and placed in a refrigerator at 10° Centigrade to incubate for three days. These specimens were agitated twice each day during this period.

TEST OF SUPERNATANT FLUID

At the end of three days the tubes were removed from the refrigerator and permitted to return to room temperature. They were then centrifuged and the supernatant fluid drawn from each by means of a tuberculin syringe. Physiological saline was then added to each specimen in the same amount and it was titrated again in three successively doubled dilutions to duplicate the original dilutions; 0.05 cc. of each dilution was then placed in a blood group tube, with fresh red cells and shaken as before at four minute intervals for thirty-

TABLE 1

SHORT TABLE OF BLOOD GROUPS OF NORTH AMERICAN POPULATIONS

People	Place	Investigators	No.	0	A	B	AB	P	q	r	D/o
ESKIMO: Nanortalik (slight mixture with Europeans)	Greenland	Fabricius-Hansen, 1939 (1)	419	48.92	44.39	4.29	2.38	.271	.034	.699	.99
Aleuts	Aleutians	Laughlin, 1948 (2)	132	48.5	44.7	4.5	2.3	.269	.032	.696	.441
NORTH AM. INDIANS: Athabascans	Peace Riv., Canada	Grant, 1936	61	62.0	38.0	0	0	.213	—	.787	—
Navajo	Ft. Defiance and St. Michaels, Ariz.	Nigg, 1926	457	72.9	26.9	0.2	0.2	.145	.001	.854	—
Stoney	Alberta, Canada	Grant, 1938	158	67.1	32.3	0	0.6	.178	—	.819	—
Blackfeet	Montana	Matson and Schrader, 1933	115	23.5	76.5	0	0	.515	0	.485	—
Blackfeet	Browning, Mont.	Matson, Levine and Schrader, 1936	103	24.3	74.8	0	1.0	.503	0	.493	—
Piegan	Brocket, Alberta	Matson, 1938	42	19.1	80.9	0	0	.563	0	.437	—
Flatheads (Salish)	W. Montana	Matson and Schrader, 1933	258	51.5	42.2	4.7	1.6	.250	.032	.718	0.3*
Salish	Coast, B. C.	Gates and Darby, 1934 and Ride and Furahata, 1935	50	80.0	20.0	0	0	.205	0	.895	—
Kwakiutl	Bella Bella Br. Columbia	Gates and Darby, 1934	94	92.6	7.4	0	0	0.37	0	.963	—

* Less than.

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(1) Laughlin, William S. Unpublished data. Peabody Museum Aleutian Expedition, 1948.
(2) All others from Boyd's published data, 1939 (a), 1939 (b).

TABLE 1—*Continued*

People	Place	Investigators	No.	0	A	B	AB	P	q	r	D/0
Tsimshian	Coast, B. C.	Gates and Darby, 1934 and Ride and Furahata, 1935	54	72.2	27.8	0	0	.150	0	.850	—
Tsimshian and Kwakiutl	Haskell Inst. Lawrence, Kans.	Landsteiner and Levine, 1929	124	66.1	33.9	0	0	.186	0	.814	—
Shoshone	S. Agency, Ft. Washakie, Wyo.	Snyder, 1926	60	51.6	45.0	1.6	1.6	.264	.011	.718	—
SOUTHWEST AND SOUTHEAST AND MEXICO											
Hopi	H. Agency, Keams Canyon, Arizona	Snyder, 1926	52	84.6	15.4	0	0	.081	0	.921	—
Pueblo, Keresan	Cochiti, Etc., N. M.	Allen and Schaeffer, 1935	353	88.0	11.7	0	0.3	.061	0	.939	—
Pueblo	N. M.	Allen and Schaeffer, 1935	307	84.5	13.3	1.6	0.6	.070	.011	.920	—
Muskhogean	Choctaw-Chickasaw Sanatorium, Talihina Oklahoma	Landsteiner and Levine, 1920	81	91.4	8.6	0	0	.044	0	.957	—
Maya	Chichén Itzá and Chankom	Goodner, 1930	223	97.7	1.3	0.5	0.5	.006	.001	.988	—

two minutes. The results were then read on the low power of the microscope. Failure of the supernatant to agglutinate the appropriate red cells is considered presumptive evidence that group substance of that group was present in the specimen and absorbed the agglutinins.

Specimens were retested and found to reduce the titer of additional amounts of sera. One specimen of 11-218 successively cleared the agglutinins from all three tubes in four successive tests.

RESULTS OF THE TESTS

Controls for these tests consisted first of known A and B stains on cloth, slightly more than one year in age, for the first test, and then previously tested skeletons were used exclusively. Four tests are described here, in which two specimens of 11-218 were tested simultaneously in two of the tests making a total of six tests for it. In these four tests the control skeleton, Khustenete No. 20, that of an Indian skeleton from the coast of Oregon, cleared all three tubes of anti-A serum in three tests and one and a half tubes in the fourth. The specimens from 11-218 cleared all three tubes of anti-A serum in all six tests. It is therefore concluded that 11-218 is of blood group A. Other controls consisted of specimens of Aleut skeletons determined to be of group A by Candela. These also gave appropriate results.

Tests with the anti-B sera, however, are not as clear. Specimens of 11-218 cleared all three tubes of anti-B in four tests. Khustenete No. 20 cleared all three tubes in two tests. In view of the fact that the skeletons typed as B by Candela did not always remove the agglutinins from three tubes in all these tests, I consider the results on B to be equivocal and therefore do not report this skeleton as AB but only as A, noting that further tests with other sera may demonstrate this to be AB.

As a final observation it may be noted that the gene for blood group A is common in the Indians of northern North America and that it is to be expected that a similarly high frequency of blood group A will be found in the skeletal remains of this area. Table 1 gives a sample distribution of blood types in North America.

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FRANKLIN'S LETTER ON INDIANS AND GERMANS*

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ONE of Benjamin Franklin's most important letters was written in 1753 on the subject of Indians and Germans in Pennsylvania. This letter amounts to a continuation of his famous *Observations concerning the Increase of Mankind, Peopling of Countries, Etc.*, which was not printed until 1755, but was written in 1751 and communicated in manuscript form to Franklin's friends Richard Jackson and Peter Collinson in 1751 or 1752. Collinson in a letter to Franklin, June 3, 1752, wrote, "Mr Jackson & myself are greatly Entertain'd in your Obs on Inc Mankind. I expect Mr Jackson will write you . . . it." In another letter July 7, 1752, he said essentially the same thing, and on September 27, 1752, he wrote, "I wish my Dear Friend you'd oblige the Ingenious Pt of Mankind with a publick View of Yr observations &c on the Increase of Mankind—I don't find any one has hit it off so well." [1] According to Professor Chinard, Franklin's supplementary letter of 1753 is "quite as important as the *Observations* to ascertain Franklin's views on the peopling of America." [2]

The original manuscript of this letter does not exist, but the text has been reprinted by Sparks, Smyth, and Van Doren from an extract from the diary of Thomas Green in the *Gentleman's Magazine* for January, 1834. An earlier version of the diary without Franklin's letter had been published in 1810 with the title *Extracts from the Diary of a Lover of Literature*. Franklin's letter appears in the diary under January 1, 1801:

> In looking over some papers this morning I met with the following curious and *unpublished letter of Dr. Franklin*, discussing some topics of considerable interest with admirable good sense and sagacity, characteristic of its author. It is dated Philadelphia May 9, 1753, and is addressed to his friend Peter Collinson Esq.

Sparks repeated this information in his edition of Franklin, but Smyth took it upon himself to declare Richard Jackson the real recipient and May 5, 1753, the real date, chiefly on the authority of a letter from Collinson to Caspar Wetstein in the British Museum. This letter, in which Collinson transcribes about one third of Franklin's letter, is labeled in the British Museum manuscript: "Ben[n] Franklin Esq[r] Philadelphia. May 5[th]. 1753." [3]

Van Doren has questioned the attribution to Jackson, chiefly because a letter of Jackson's, March 17, 1754, which acknowledges Franklin's "favour of the 5th of May" has nothing to do with population or Germans. Van Doren, therefore, reprints the text of the *Gentleman's Magazine* and restores May 9 as the date and Collinson as the recipient.[4]

Another and more complete version of the letter, however, exists—a manuscript version among the Shaftesbury papers of the London Public Records Office.[5] In this version Collinson is given as the recipient and the date is May 9, 1753. It demands printing because of several variations from the other texts. In the Green version in the *Gentleman's Magazine* an omission in the middle of the letter is indicated by asterisks. The omitted material is restored in the Shaftesbury version, printed below, along with a significant conclusion, found in no other version, in which Franklin suggests that liberty may come to thrive in America if it is reduced in Britain. Here we also find completed and identified a reference to Milton about which editors of the incomplete version have been forced to speculate.

Philadelphia May y[e] 9[th]. 1753

Sir.

I received your favour of the 29[th] August last, & thank you for the kind and Judicious Remarks you have made on my little piece; Whatever further Oc-

* This note was written with the aid of a grant from the American Philosophical Society.

[1] Franklin Papers in the American Philosophical Society.

[2] Eighteenth century theories on America as a human habitat, *Proc. Amer. Philos. Soc.* **91**: 56, 1947.

[3] Papers of Caspar Wetstein. Add. 32420.

[4] *Letters and papers of Benjamin Franklin and Richard Jackson*, Phila., *Mem. Amer. Philos. Soc.* **24**: 31–40, 1947.

[5] Shaftesbury Papers. (G. D. 24) Bdle. 49. No. 24.

curs to you on the same Subject, you will much oblige me in communicating it.

I have often observed with wonder, that Temper of the poorer English Manufacturers and Day Labourers which you mention, and acknowledge it to be pretty general. When any of them happen to come here, where Labour is much better paid then in England, their Industry seems to diminish in equal proportion. But it is not so with the German Labourers; They retain the habitual Industry and frugality they bring with them, and now receiving higher Wages, an accumulation arises that make them all rich.

When I consider, that the English are the offspring of Germans, that the Climate they live in is much of the same Temperature; When I can see nothing in Nature that should create this difference, I am apt to Suspect it must arise from Institution, And I have sometimes doubted, whether the Laws peculiar to England, which compel the Rich to maintain the Poor, have not given the latter a Dependance, that very much lessens the Care of providing against the wants of Old Age.

I have heard it remarked that the Poor in Protestant Countries on the Continent of Europe, are generally more Industrious than those of Popish Countries, may not the more Numerous foundation in the Latter for relief of the Poor have some effect towards rendring them less providant. To relieve the Misfortunes of Our Fellow Creatures is concerning with the Diety, 'tis Godlike, But if we provide Encouragements for Laziness, and Support for Folly, may it not be found Fighting against the Order of God and Nature, which perhaps has appointed Want and Misery as the proper Punishments for and Cautions against, as well as necessary Consequences of Idleness and Extravagancy, Whenever we attempt to mend the Scheme of Providence and to interfere in the Government of the World, we had need be very circumspect, lest we do more harm than good. In New England they Once thought Blackbirds Useless and mischieveous to their Corn, they made Laws to destroy them, the Consequence was; the Blackbirds were diminish'd, but a kind of Worms which devour'd their Grass, and which the Blackbirds had been used to feed on encreased prodigiously; Then finding their Loss in Grass much greater, then their saving in Corn, they wish'd again for their Blackbirds.

We had some Years since a Transylvanian Tartar, who had Travelled much in the East, and came hither merely to see the West, intending to go home thro' the Spanish West Indies, China, &c.— He asked me One Day what I thought might be the reason that so many and such numerous Nations, as the Tartars in Europe and Asia, the Indians in America, and the Negroes in Africa, continued a Wandring Careless Life, and refused to Live in Cities, and to Cultivate the Arts they saw practized by the civilized part of Mankind— While I was considering what Answer to make him; I'll tell you Says he in his broken English, *God make Man for Paradise, he make him to live Lazy; Man make God angry, God turn him Out of Paradise, and bid work, Man no love work, he want to go to Paradise again, he want to Live Lazy; So all Mankind love Lazy.* However this may be it seems certain, that the hope of becoming at sometime of Life free from the necessity of Care & Labour, together with fear of Penury, are the main Springs of most Peoples Industry. To those indeed who have been educated in Elegant plenty, even the provision made for the poor may appear Misery, But to those who have scarce ever been better provided for, such provision may seem quite good and Sufficient, these latter have then nothing to fear worse than their present Conditions, and Scarce hope for any thing better than a Parish Maintenance; So that there is only the difficulty of getting that Maintenance allowed while they are able to work, or a little Shame they Suppose attending it, that can induce them to work at all, and what they do will only be from hand to Mouth.

The Proneness of Human Nature to a Life of Ease, of Freedom from Care and Labour appears Strongly in the Little Success that has hitherto attended every attempt to Civilize Our American Indians in their present way of Living, almost all their Wants are supplied by the Spontaneous Productions of Nature, with the additions of very little Labour, if Hunting and Fishing may indeed be called Labour when Game is so plenty, They Visit us frequently, and see the advantages that Arts, Sciences, and compact Societies procure Us, they are not deficient in Natural Understanding and yet they have never shown any inclination to change their Manner of Life for Ours, or to Learn any of Our Arts. When an Indian Child has been brought up among Us, taught our Language and habituated to Our Customs, yet if he goes to see his Relations and make one Indian Ramble with them, there is no perswading Him ever to return, And that this is not natural to them merely as Indians, but as Men, is plain from this, that when White Persons of either Sex have been taken Prisoners Young by the Indians, and lived a while among them, tho' ransom'd by their Friends, and treated with all imaginable tenderness to prevail with them to stay among the English, yet in a Short time they become disgusted with Our Manner of Life, and the care and Pains that are necessary to Support it, and take the first good opportunity of Escaping again into the Woods, from whence there is no reclaiming them. One Instance I remember to have heard, where the Person was brought Home to possess a good Estate, but finding some Care necessary to keep it together, he relinquish'd it to a Younger Brother, reserving to himself nothing but a Gun & a Match Coat, with which he took his way again to the Wilderness. They have few but natural wants, and those easily supplied; But with us are infinite Artificial Wants, no less craving then those of Nature, and much more difficult to Satisfie; So that I am apt to imagine that close Societies subsisting by

Labour and Art, arose first, not from choice, but from necessity, When Numbers being driven by War from their Hunting Grounds, and prevented by Seas or by other Nations were crouded together into some Narrow Territories, which without Labour would not afford them Food, However as matters now stand with Us, Care and Industry seem absolutely necessary to Our well being, They should therefore have every Encouragement we can invent, and not one motive to Diligence be subtracted, and the Support of the Poor should not be by maintaining them in Idleness, but by Employing them in some Kind of Labour suited to their abilities of Body &c. As I am informed begins of late to be the practice in many parts of England, where work Houses are Erected for that purpose. If these were general I should think the Poor would be more carefull and work voluntarily to lay up something for themselves against a rainy Day, rather than run the risque of being obliged to work at the pleasure of others for a bare Subsistance, and that too under Confinment, The little Value Indians set on What we prize so highly under the Name of Learning, appears from a pleasant Passage that happen'd some years since at a Treaty between One of Our Colonies and the Six Nations, When every thing had been Settled to the Satisfaction of both sides, and nothing remain'd but a Mutual Exchange of Civilities, the English Comissioners told the Indians, they had in their Country a College for the instruction of Youth, who were there taught various Languages, Arts, and Sciences, that there was a particular Foundation in favour of the Indians to defray the Expence of the Education of any of their Sons, who should desire to take the benefit of it, and now if the Indians would accept the offer, the English would take half a dozen of their brightest Lads, and bring them up in the best Manner; The Indians after consulting on the Proposal, replied, That it was remembrd some of their youths had formerly been Educated in that College, but it had been observed that for a long time after they returned to their Friends, they were absolutely good for nothing, being neither acquainted with the true methods of Killing Deer, catching Beaver, or Surprizing an Enemy, The Proposition however, they look'd on as a Mark of the Kindness and good will of English to the Indian Nations, which merited a gratefull return, and therefore if the English Gentlemen would send a Dozen or two of their Children to Onondago, the great Council would take Care of their Education, bring them up in really what was the best Manner and make Men of them. I am perfectly of your Mind, that Measures of great Temper are necessary with the Germans, and am not without apprehensions, that thro' their Indiscretion or Ours, or both, great disorders and inconveniences may one Day arise among Us, those who come hither are generally of the most ignorant Stupid Sort of their Own Nation, and as ignorance is often attended with Credulity when Knavery would misled it, and with Suspicion when Honestly would set it right, and as few of the English understand the German Language, and so cannot address them either from the Press or Pulpit, 'tis almost impossible to remove any prejudices they once entertain; Their Own Clergy have very little influence over the People, who seem to take an uncommon pleasure in abusing and Discharging the Minister on every trivial Occasion, not being used to Liberty, they Know not how to make a Modest Use of it, and as *Kolben* says of the Young Hottentots, that they are not esteemed Men till they have shown their Manhood by beating their Mothers, so these seem not to think themselves free, till they can feel their Liberty in abusing and insulting their Teachers. Thus they are under no restraint of Ecclesiastical Government, they behave, however, submissively enough at present to the Civil Government which I wish they may continue to do: For I remember when they modestly declined intermedling in Our Elections, but now they come in droves, and carry all before them, except in One or two Counties, Few of their Children in the Country learn English, they import many Books from Germany, and of the Six printing Houses in the Province, two are entirely German, two half German half English, and but two entirely English; they have one German News Paper, and one half German advertisements intended to be general are now printed in Dutch and English; The Signs in Our Streets have Inscriptions in both Languages, and in Some places only German, they begin of late to make all their Bonds and other Legal Writings in their own Language, with (tho' I think it ought not to be) are allowed good in Our Courts, where the German Business so encreases, that there is continual need of Interpreters, and I suppose in a few Years they will be also necessary in the Assembly, to tell one half of Our Lesgislators what the other half say; In short unless the Stream of their Importation could be turn'd from this to other Colonies, as you very judiciously propose, they will soon so out Number Us, that all the advantages we have, will not in my Opinion be able to foreserve Our Language, and even Our Government will become precarious. The French who watch all advantages, are now themselves making a German Settlement back of Us in the Illinois Country, and by means of those Germans they may in time come to an Understanding with Ours, And indeed in the last War Our Germans Show'd a general Disposition that seems to bode Us no good, For when the English who were not Quakers, alarm'd by the danger arising from the defenseless State of Our Country enter'd Unanimously into an Association within this Government, and the Lower Countries rais'd arm'd and Disciplin'd near 10,000 Men, the Germans except a very few in Proportion to their Numbers refused to engage in it, giving out one among another, and even in print, that if they were quiet, the French, should they take the Country would not molest them; At the same time abusing the Philadelphians for fit-

ting out Privateers against the Enemy; And representing the trouble Hazard and Expence of defending the Province as a greater Inconvenience then any that might be expected from a Change of Government.—Yet I am not for refusing entirely to admit them into Our Colonies: All that seems to be necessary is, to distribute them more equally, mix them with the English, establish English Schools where they are now too thick Settled, and take some care to prevent the practice lately fallen into by some of the Ship Owners, of Sweeping the German Goals to make up the Number of their passengers. I say I am not against the admission of Germans in general, for they have their Virtues, their Industry and Frugality is exemplary, they are excellent Husbandmen and contribute greatly to the Improvement of a Country.

I pray God long to preserve to Great Britain the English Laws, Manners, Liberties and Religion notwithstanding the Complaints so frequent in your publick Papers of the prevailing Corruption and Degeneracy of your People. I know you have a great deal of Virtue still subsisting among you, and I hope the Constitution is not so near a dissolution, as some seems to apprehend, I do not think you are generally become such Slaves to your Vices, as to draw down that *Justice* Milton speaks of When He says that sometimes Nations will descend so low from Reason which is Virtue, that no Wrong, But Justice, and some fatal Curse annex'd deprives them of their outward Liberty.

Their inward lost . . . . Parad. lost

In History we find that Piety, Publick Spirit, & Military Prowess have their Flows, as well as their Ebbs, in every Nation, and that the tide is never so low but it may rise again, But should this dreaded fatal Change happen in my time, how should I even in the midst of the Affliction rejoice, if we have been able to preserve those invalueable Treasures, and can invite the good among you to come and partake of them! O let not Britain seek to oppress Us, but like an Affectionate Parent endeavour to secure Freedom to her Children; They may be able one Day to assist her in Defending her Own—Whereas a Mortification begun in the Foot may spread upwards to the Destruction of the nobler Parts of the Body.

I fear I have already extended this rambling Letter, beyond your Patience, and therefore conclude with requesting your Acceptance of the inclosed Pamphlet, from

Sir

Your most humble Servt

Discovery and printing of this version does not solve all the problems concerning the letter. Franklin begins by thanking Collinson for the "favour of the 29th August last." Among the Collinson letters preserved in Franklin's correspondence in the American Philosophical Society there is no letter written on this date, although there are letters on the subject of population written on June 3, July 7, August 12, and September 27, 1752. In the letter of August 12, Collinson reports that Franklin's "Impartial Acct of the State of the Germans came very Seasonably to awaken the Legislature to take some Measures to Check the Increase of these Powers." Perhaps Franklin was replying to this letter and the transcriber of the copy in the Shaftesbury papers made a mistake in the date, or perhaps Collinson wrote another letter on August 29 which has not been preserved.

On the connection of Jackson with the letter, it may be said that it is possible, but not likely, that Franklin wrote to Jackson on May 5 and then repeated the gist of his remarks to Collinson on May 9. It is difficult to believe that Collinson in writing to Wetstein would transcribe remarks from a letter to Jackson which were duplicated almost verbatim in a letter to himself. One of Franklin's references, moreover, is clearly to Collinson rather than to Jackson: "In short unless the Stream of their Importation could be turn'd from this to other Colonies, as you very judiciously propose, they will soon so out Number Us, that all the advantages we have, will not in my Opinion be able to foreserve Our Language." "As you very judiciously propose" is almost certainly a reference to Collinson's "Hints Humbly proposed to Incorporate the Germans more with the English & Check the Increase of their power," enclosed with a letter to Franklin, August 12, 1752. Collinson's seventh and last hint is "To Discourage the sending more Germans to the Pro: of Pensilvania When Inhabitants are so much Wanted in Georgia, North Carolina & Nova Scotia—."[6] This seems to indicate clearly that Collinson alone was the original recipient.

For the sake of completeness, it is appropriate to add the concluding paragraph of Collinson's letter to Wetstein, which has never before been published.

Thus my Dear Friend I have transcribed what Relates to the Germans It seems very Necessary that Some Immediate Steps be Soone taken to Disperse them more in the Colonies & to Incorporate them more & more with the English, If they would be Induced to Intermarry more with the English & the English with them by granting them some privileges for so doing might be a great Means to break the Connection they have one with Another—undoubtedly the Erecting English Schools will be of Great Service—and what would Induce them to Learn English *if an*

[6] Franklin Papers in the American Philosophical Society.

Act of parliament passed Here, that no German should be Capable to take & Enjoye any post with the Government Even so Low as a Constable unless He & His Family could Speak English.

Here Collinson incorporates four of the seven "Hints . . . to Incorporate the Germans" which he had sent to Franklin on August 12, 1752. The remaining three hints sent to Franklin concern prohibiting German legal documents, suppressing German printing houses, and prohibiting the importation of German books.

Smyth's version of Franklin's letter follows neither that in the *Gentleman's Magazine* nor that in the British Museum, but is apparently a hybrid of his own manufacture. He suggests that in his version he follows the letter among the Wetstein papers from the passage about the Germans beginning "I am perfectly of your mind . . ." to the end of the letter. Actually he does no such thing. The Wetstein letter, in the first place, does not even have the last paragraph of the Smyth version, but concludes the extract from Franklin with "Contribute Greatly to the Improvement of a Country." Smyth does not follow the Wetstein letter accurately even in the sections in which it is devoted to Franklin. The passage quoted above, referring to Collinson's "Hints," for example, appears in the Wetstein letter:

> In Short unless the Stream of their importation could be turned from This, to other Colonies as you S^r very Judiciously propose, they will Soon So outnumber us. That all the Advantages We have, will not, in my Opinion, be able to preserve our Language.

Smyth prints it:

> In short, unless the Stream of their Importation could be turned from this to other Colonies, as you very judiciously propose, they will soon so outnumber us, that all the advantages we have, will not in my Opinion be able to preserve our Language.

Since there are three different versions of Franklin's letter, some decision must be made as to which is closest to the original. We must choose between the Thomas Green version in the *Gentleman's Magazine* (followed by Sparks and Van Doren), the Wetstein version (followed in part by Smyth), and the Shaftesbury version (printed above). Perhaps one should say that there are four versions, for the text produced by Smyth not only changes the spelling and punctuation of the Wetstein version, but leaves out whole words. This version may be safely ignored.

Since the Shaftesbury version is the longest and most complete, it must obviously be accepted as authoritative for the sections not contained in the other versions and should be used as a framework for the other sections. If the assumption is correct that the original letter was written to Collinson, not to Jackson, and since the Wetstein letter is in Collinson's handwriting, it follows that for this section of the letter the Wetstein version is the most exact. In spelling and punctuation, however, this version is not at all reliable, for Collinson was uniformly careless and erratic in mechanics, whereas Franklin was ordinarily careful and consistent. In general, the Wetstein version is closer to the Shaftesbury than it is to the Green, but it agrees variously with one and then the other, and at times with neither. Most of the differences are caused either by changes in minor parts of speech or by synonyms for important words. The Shaftesbury version seems to be incorrect in using the word "foreserve" in the phrase "all the advantages we have, will not in my Opinion be able to foreserve Our Language." The other two versions use "preserve."

In the rest of the letter, not duplicated by the Wetstein extract, the Shaftesbury version seems to be inexact at least once. The Shaftesbury version reads: "To relieve the Misfortunes of Our Fellow Creatures is concerning with the Diety, 'tis Godlike." The Green version is probably Franklin's: "concurring with the Deity." The Shaftesbury version speaks of white men, reverting to a savage life in the woods, "from whence there is no reclaiming them." The Green version speaks of "redeeming them." It is hard to say which Franklin meant.

At least once the Shaftesbury version is more accurate than the Green. The latter speaks of sending white children to an Indian settlement, "Opondago." The Shaftesbury version gives the correct name, "Onondago," a name which appears elsewhere in Franklin's works.

One humorous passage in the Green version is less comic in the Shaftesbury version. The Transylvania Tartar says in the former version: "God make man for Paradise. He make him for live lazy. Man make God angry. God turn him out of Paradise, and bid workee. Man no love workee; he want to go to Paradise again; he want to live lazy." In the Shaftesbury version, the Tartar uses broken English, but not pidgin English. *"God turn him Out of Paradise, and bid work, Man no love work, ..."* In his Information to Those Who Would Remove to America, Franklin has a Negro say that white men *"make de black man workee, make de Horse workee, make de Ox workee, make ebery ting workee. . . ."* Would Franklin use the same dialect for an Indian and a Negro?

FRANKLIN AND JACKSON ON THE FRENCH WAR*

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In one of his letters written late in life to his son, Franklin expressed disappointment that he had not been received by William Pitt when he sought an interview with the famous statesman in 1757.

> When I came to England in 1757, you may remember I made several Attempts to be introduc'd to Lord Chatham (at that time first Minister), on account of my Pensilvania Business, but without Success. He was then too great a Man, or too much occupy'd in Affairs of greater Moment. I was therefore oblig'd to content myself with a kind of nonapparent and unacknowledg'd Communication thro' Mr. Potter and Mr. Wood, his Secretaries, who seem'd to cultivate an Acquaintance with me by their Civilities, and drew from me what Information I could give relative to the American War, with my Sentiments occasionally on Measures that were proposed or advised by others, [which gave me the Opportunity of recommending and enforcing the Utility of conquering Canada.] I afterwards considered Mr. Pitt as an *Inaccessible*. I admired him at a distance, and made no more Attempts for a nearer Acquaintance.[1]

The information which Franklin gave to Pitt's secretaries at this or at other meetings eventually came to Pitt's personal attention, for a document ascribed jointly to Franklin and Richard Jackson is now in the Chatham Papers of the London Public Records Office.[2]

In May, 1756, England declared war against France. Even before this, the colonists had been planning military campaigns. Governor William Shirley of Massachusetts, for example, had early in the year asked the provinces for troops and money to be used in driving the French out of their forts on Lake Ontario and to attack Ticonderoga, Crown Point, and Fort Duquesne.[3] When Pitt became Secretary of State in June, 1757, he began collecting information and intelligence reports from colonial governors and others. "Like many other men conscious of supreme ability, Pitt tended to concentrate the whole business of the office in his own hands."[4] Among his informants on the military situation in America were Denys de Berdt, William Bollan, Sir Theodore Janssen, Governor Pownall, James Abercromby, Samuel Waldo, and Ephraim Biggs. On September 5, 1757, Colonel John Bradstreet presented a proposal similar in some respects to Franklin's and Jackson's.[5] Bradstreet proposed first that the pro-French Indians be attacked at once and brought to submission; second that Fort Niagara and Fort Duquesne be captured in the summer of 1758, only four thousand men to be employed in the operation. His other four points concern the raising and distribution of troops and their subsequent use against Ticonderoga and Montreal.

By December 30, 1757, Pitt had considered the advice of his various informants and had decided on the strategy to be followed. In a circular letter of this date sent to the governors in North America, he announced the campaign for the following year.[6]

Franklin's and Jackson's joint set of proposals must have been read by Pitt and weighed with the opinions of other informants. They were probably submitted between June, 1757, when Franklin sailed for England as agent for the Pennsylvania assembly, and December 30, 1757, when Pitt announced his campaign, although it is possible that they were drawn up during the progress of the campaign in 1758. They may have been solicited informally by Pitt's secretaries, Potter and Woods, as Franklin suggested in his letter to his son, or they may have been prepared as a formal consideration of proposals already made by other authorities, Franklin and Jackson serving as a sort of superior authority. The phrases in the heading of the document suggest the latter.

* This note was written with the aid of a grant from the American Philosophical Society.

[1] Smyth, A. H., ed., *Writings of Benjamin Franklin* **6**: 320–321, N. Y., Macmillan, 1907.

[2] Gifts and Deposits. Chatham Papers (GD 8) Bdle 96 (Extracts).

[3] Kimball, Gertrude S., ed., *Correspondence of William Pitt . . . with Colonial Governors* **1**: xxvii, N. Y., Macmillan, 1906.

[4] Tunstall, Brian, *William Pitt, Earl of Chatham*, 188, London, Hodder, 1938.

[5] *Ibid.*, 200 ff.

[6] Kimball, *op. cit.*, **1**: 136–40.

Mr. Franklin & Mr. Jackson have consider'd the Points props'd to them, & are of Opinion, That.

In case the Winter Expedition against Fort Duquesne prove effectual, the two Forts of Beef River and Presqu'isle should be taken early in the Spring, and (if we do not intend a Naval Force on Lake Erie) they should be both destroy'd; if we do, they should be both strengthen'd. They are Posts of no great strength at present, and without Canon, unless supply'd since the Summer 1756. The Garrison in each consisted then but of 30 Men. Battoes can go up, during the Spring Freshet, quite to Beef River Fort, and there is only 15 Miles Land Carriage from thence to Presqu'isle on Lake Erie; a very good Road.

If Certain Intelligence is obtain'd at Presqu'isle, that the French have left only a weak Garrison at Niagara, as expecting the English at Quebeck or Montreal; (which is Likely, for in 1756. they drew off the Garrison that had been plac'd there to defend that Fort against Gen[l] Shirley, and left only 50 Men:) in that Case, Niagara might be attempted from Presqu'isle, and those Forts preserv'd till that Expedition is over. If Niagara is taken, these should both be preserv'd to keep up the Communication; for Niagara itself should be kept and maintained by a strong Garrison and well Supply'd, it being a better Harbour for Naval Force than Oswego. Presqu'isle should also be strengthen'd and made a Port for Naval Force on Lake Erie. The taking & keeping Fort Niagara would give us great Reputation & Influence with the Six Nations.

If this be not attempted, at least strong Ranging Parties should be sent out from Fort Duquesne to annoy the Shawnese and other Indians that have join'd the French against us, Destroy their Towns, and Oblige them to sue for Peace or quit the Country; & to destroy the French Posts between Lake Erie & Ohio. Much of this kind may be done this Summer by the Forces that take Duquesne, if they have diligent and active Officers.

As the Frontiers of Virginia, Maryland, Pensilvania and New Jersey will be render'd pretty secure and quiet by the Taking of Duquesne, the Prc vincial Forces of those Colonies, amounting to above 2000 Men, might be almost all employ'd in those small subsequent Expeditions.

Means should also be used to open again our Intercourse with the Twigtwees, and engage them to declare for us. They are Numerous & strong, and enclin'd to the English. The Destroying the French Posts in their Country is Necessary to this End. Their Trade with us would soon be very Considerable & beneficial. If we take the Fort at Pontchartrain, or Detroit, all the Tribes of the Twigtwees will be secured to us, and must depend upon us.

The actual campaign did not proceed with the celerity which Pitt hoped, and consequently Franklin's and Jackson's points were not followed to the letter. The fault was not in the plans themselves, but rather in the difficulty of communicating and revising them.

It is some extenuation of Pitt in 1758 that his plans had to be based upon insufficient information; he had before him in December the winter quarters of the troops as they were determined in the autumn of 1757, and not as Loudoun moved them during the course of the winter. But the very fact that it took six months for an exchange of information and orders was an inherent weakness of Pitt's policy; half the year was lost.[7]

Major General Abercromby was appointed commander-in-chief of the king's forces in America, and Brigadier General Forbes was given charge of the operation against Fort Duquesne. Forbes' progress was impeded by illness and by his insistence on rigidly following accepted military rules and discipline. He did not come within striking distance of Fort Duquesne until November, 1758, and even then decided not to proceed. It was Washington who urged the necessity of going ahead, and who led the advance brigade. On November 25, 1758, Washington occupied Fort Duquesne.

Two days later Forbes gave Pitt credit for the undertaking. "So give me leave to congratulate you upon this great Event, of having totally expelled the French from this prodigious tract of Country, and of having reconciled the various tribes of Indians inhabiting it to His Majesty's Government."[8] Perhaps to account for his failure in carrying out plans to the letter, Forbes adds, "I should have carried the troops up the River to the Lake Erie, and destroyed the French posts at Venango and Presque Isle, but the Season of the Year, and the Scarcity of my Provisions, does by no Means admitt of it."

Even if Franklin's and Jackson's advice had been followed promptly, completely and successfully, their statement would not be one of the most vital documents in the history of colonial America. It has some value, nevertheless, as one of many documents of secondary importance, ancillary to a major historical event. The document is important in Franklin's biography since it provides definite evidence of the close relations between Franklin and Jackson in the period between 1757 and 1762, of which otherwise "only a few incidental traces exist."[9]

[7] Pargellis, Stanley M., *Lord Loudoun in North America*, 360, New Haven, Yale Univ. Press, 1933.

[8] Kimball, *op. cit.*, **1**: 406.

[9] Van Doren, Carl, *Letters and papers of Benjamin Franklin and Richard Jackson*, Phila., *Mem. Amer. Philos. Soc.* **24**: 16, 1947.

FRANKLIN'S DEISTICAL INDIANS[1]

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BENJAMIN FRANKLIN'S *Remarks concerning the Savages of North America*, a work published late in the author's life, 1784, is composed of elements from three other works published from twenty to sixty years previously.[2] One of these is a Latin discourse published in Sweden by the son of a returned missionary; another an Indian treaty printed by Franklin at his Philadelphia press; and the third an ingenious literary hoax perpetrated by Franklin in a London newspaper. Not a mere bagatelle, but an artistic achievement, this literary hoax consists of a fictitious narrative of Indian captivity in the vein of *Gulliver's Travels*, a work which has escaped all of Franklin's editors and critics.

Franklin's varied purposes in writing the *Remarks* are not readily apparent on the surface, and his critics and biographers have failed to accord the work the analysis and appraisal that it merits. From a purely literary consideration, the *Remarks concerning the Savages* is one of the most clever and original from Franklin's pen. As we examine his sources we shall see Franklin in a new light—as a satirist of religious proselytizing and economic imperialism. The newly-discovered prose narrative in which he indulges his satirical vein helps us understand the combination of satire and realism which constitutes the later *Remarks*.

Much is communicated by Franklin's introductory sentence. "Savages we call them, because their Manners differ from ours, which we think the Perfection of Civility: they think the same of theirs." Primitivism is the theme of this sentence; inculcation of tolerance the purpose; and satire the suggested method. In works of this genre, the contrast between western civilized society and a primitive or exotic one causes the reader to observe the manners and conduct of his own society from a universal point of view and to find his own habits and customs no less ridiculous than primitive ones. Franklin transports the theme of Montesquieu's *Lettres persanes* and Goldsmith's *Citizen of the World* to the western hemisphere. In studying his *Remarks concerning the Savages*, we may try to decide how much of his inspiration is purely literary, like Montesquieu's and Goldsmith's, and how much is based upon his actual experience with Indians, which, his biographers have shown, was direct and extensive.

All sections of Franklin's work reflect his desire to give both a true and essentially attractive portrait of the Indian character. Although primarily a series of humorous character sketches, the *Remarks* has an underlying serious purpose of indirectly urging that justice be done to the Indians. The serious side of the *Remarks* may be interpreted in the light of a statement by Franklin in a letter to Samuel Elbert in which he says, "During the Course of a long Life in which I have made Observations on public Affairs, it has appear'd to me that almost every War between the Indians and Whites has been occasion'd by some Injustice of the latter towards the former." [3]

To illustrate the importance of oratory among the Indians and their contempt for European learning, Franklin tells at the outset of his work the story of an offer made by some Virginia gentlemen to educate a dozen Indian youths at Williamsburg. The Indians receive the offer with politeness and, although they reject it, offer in return to take a dozen sons of the Virginia gentlemen and make men of them. Next Franklin discusses the decorum of the Indians in council, the phenomenal memories of their women, and the extreme politeness of all Indians in conversation. To show the latter quality, he describes the efforts made by a Swedish minister to convert an assembly of Susquehanna Indians. The minister delivers a sermon, containing all of the fundamental doctrines of the Christian faith. When he finishes, an Indian orator stands up and recounts a cosmogony from Indian mythology. When the minister rebukes him for telling idle tales, the In-

[1] This study was undertaken with the support of a grant from the Penrose Fund of the American Philosophical Society.

[2] A. H. Smyth, ed., *Writings of Benjamin Franklin* **10**: 97–105, N. Y., Macmillan, 1907.

[3] December 16, 1787. Smyth, **9**: 625.

dian reproaches the minister in return for not believing the Indian legends after they had set him the example of politely believing his. As a final illustration of Indian politeness, Franklin describes their generous hospitality and contrasts it with the greed of the whites, who constantly cheat the Indians in trading.

The opening section of the *Remarks,* the narrative concerning the Indian attitude toward western learning, is based on the report of an Indian treaty which Franklin himself had published at his Philadelphia press.[4] At this treaty, held at Lancaster in 1744, the Governor of Pennsylvania, George Thomas, parleyed with the Indians. Replying to an Indian speech praising the interpreter at the proceedings, Conrad Weiser, the Governor, remarked that when Weiser became old he would pass into another world, and that the Indians would then need another such friend to serve as interpreter.

> The Way to have such a Friend, is for you to send three or four of your Boys to *Virginia,* where we have a fine House for them to live in, and a Man on purpose to teach the Children of you, our Friends, the Religion, Language and Customs of the white People. To this Place we kindly invite you to send some of your Children, and we promise you they shall have the same Care taken of them, and be instructed in the same Manner as our own Children, and be returned to you again when you please.

To this proposal and others which had been made, the Indian speaker, an Onondago chief named Canassatego, replied:

> We return you Thanks for your several Speeches, which are very agreeable to us. They contain Matters of such great Moment, that we propose to give them a very serious Consideration, and to answer them suitably to their Worth and Excellence; and this will take till To-morrow Morning, and when we are ready we will give you due Notice.

On the following day, Canassatego refused politely, but firmly, the offer to educate Indian children:

> We must let you know we love our Children too well to send them so great a Way, and the *Indians* are not inclined to give their Children Learning. We allow it to be good, and we thank you for your Invitation; but our Customs differing from yours, you will be so good as to excuse us.

This scene must have made a vivid impression upon Franklin, for nine years later he gave a lengthy report of the proceedings in a letter to Peter Collinson, May 9, 1753.

> The little Value Indians set on what we prize so highly under the Name of Learning, appears from a pleasant Passage that happen'd some Years since at a Treaty between One of Our Colonies and the Six Nations. When everything had been Settled to the Satisfaction of both sides, and nothing remain'd but a Mutual Exchange of Civilities, the English Commissioners told the Indians, they had in their Country a College for the instruction of Youth, who were there taught various Languages, arts, and Sciences, that there was a particular Foundation in favour of the Indians to defray the Expence of the Education of any of their Sons, who should desire to take the benefit of it, and now if the Indians would accept the offer, the English would take half a dozen of their brightest Lads, and bring them up in the best Manner; The Indians, after consulting on the Proposal, replied, That it was remember'd some of their Youths had formerly been Educated in that College, but it had been observed that for a long time after they returned to their Friends, they were absolutely good for nothing, being neither acquainted with the true method of Killing Deer, catching Beaver, or Surprizing an Enemy, The Proposition however, they look'd on as a Mark of the Kindness and good will of English to the Indian Nations, which merited a gratefull return, and therefore if the English Gentlemen would send a Dozen or two of their Children to Onondago, the great Council would take Care of their Education, bring them up in really what was the best Manner and make Men of them.[5]

Franklin has embellished the narrative taken from the treaty and added the all-important counter-offer of the Indians and the information that some Indian youths had previously been educated by the English—details apparently originating in Franklin's imagination.

In his *Remarks concerning the Savages of North America,* Franklin tells the same story, but adapting it to his primary theme of Indian politeness, emphasizes the droll aspects of the scene.

> Our laborious Manner of Life, compared with theirs, they esteem slavish and base; and the Learning, on which we value ourselves, they regard as frivolous

[4] *A Treaty, Held at the Town of Lancaster, in Pennsylvania . . . with the Indians of the Six Nations, in June, 1744* (Philadelphia, 1744), reprinted in *Indian treaties printed by Benjamin Franklin 1736–1762.* With an Introduction by Carl Van Doren and Historical and Bibliographical Notes by Julian P. Boyd, 72 ff., Phila., Hist. Soc. Penna., 1938.

[5] The best printed version of the letter to Collinson appears in Carl Van Doren, *Letters and papers of Benjamin Franklin and Richard Jackson 1753–1785,* Phila., *Mem. Amer. Philos. Soc.* **24**: 31–40, 1947. The excerpt above is taken from a manuscript version found in the London Public Records Office.

and useless. An Instance of this occurred at the Treaty of Lancaster, in Pennsylvania, *anno* 1744, between the Government of Virginia and the Six Nations. After the principal Business was settled, the Commissioners from Virginia acquainted the Indians by a Speech, that there was at Williamsburg a College, with a Fund for Educating Indian youth; and that, if the Six Nations would send down half a dozen of their young Lads to that College, the Government would take care that they should be well provided for, and instructed in all the Learning of the White People. It is one of the Indian Rules of Politeness not to answer a public Proposition the same day that it is made; they think it would be treating it as a light matter, and that they show it Respect by taking time to consider it, as of a Matter important. They therefore deferr'd their Answer till the Day following; when their Speaker began, by expressing their deep Sense of the kindness of the Virginia Government, in making them that Offer; "for we know," says he, "that you highly esteem the kind of Learning taught in those Colleges, and that the Maintenance of our young Men, while with you, would be very expensive to you. We are convinc'd, therefore, that you mean to do us Good by your Proposal; and we thank you heartily. But you, who are wise, must know that different Nations have different Conceptions of things; and you will therefore not take it amiss, if our Ideas of this kind of Education happen not to be the same with yours. We have had some Experience of it; Several of our young People were formerly brought up at the Colleges of the Northern Provinces; they were instructed in all your Sciences; but, when they came back to us, they were bad Runners, ignorant of every means of living in the Woods, unable to bear either Cold or Hunger, knew neither how to build a Cabin, take a Deer, or kill an Enemy, spoke our Language imperfectly, were therefore neither fit for Hunters, Warriors, nor Counsellors; they were totally good for nothing. We are however not the less oblig'd by your kind Offer, tho' we decline accepting it; and, to show our grateful Sense of it, if the Gentlemen of Virginia will send us a Dozen of their Sons, we will take great Care of their Education, instruct them in all we know, and make Men of them.[6]

In the subsequent section of the *Remarks,* Franklin in like manner extracts the droll elements from another episode in Indian history and presents them in a crisp anecdote. Franklin's account of a Swedish minister and his unsuccessful proselytizing among the Indians is based directly on a widely-circulated deistical essay, which in turn is based on an actual Indian oration published in Latin by the son of a Swedish missionary whose friend delivered the sermon. The oration appears in a Latin graduation dissertation *Concerning the Planting of the Swedish Church* in America presented for examination in 1731 by Tobias Eric Biörck to Upsala University.[7] The original missionary, John Auren (Aurenius), delivered a sermon in 1699 or 1700, setting forth the fundamental doctrines of Christianity, to some Indians at Canistoga. An Indian spokesman in reply subjected these doctrines to examination and found them deficient. The natural abilities of this Indian orator made such a favorable impression upon Auren that he made a faithful record of the whole discourse while it was fresh in his memory and submitted it to his friend the Reverend Eric Biörck on January 13, 1700. Biörck, a missionary-pastor in Delaware from 1697 until his return to Sweden in 1714, turned the discourse over to his son Tobias, who included it in his Latin history of the Swedish churches in America when he presented it in 1731. Biörck's book is so rare that only two copies are known to be in American libraries, yet some enterprising deist discovered the oration it contains, translated it literally, and worked it into a persuasive essay, showing the superiority of natural religion to the hollow formalism of Christianity. Because of the rarity of Biörck's book, this English version of the Indian oration has been generally regarded as a malicious fiction fostered by infidels. Franklin probably never saw the Latin dissertation, but knew the deistical essay based upon it.[8]

His acquaintance with the deistical piece is attested by his correspondence concerning James Parker, publisher of the *New York Gazette.* The latter had been indiscreet enough to print the piece (April 27, 1752, No. 484), and when he found himself in a great deal of trouble as a result, he appealed to Franklin to intercede in his behalf. Franklin in a letter to Cadwallader Colden had the following remarks on the influence of the essay.

[6] Smyth, **10**: 98–99.

[7] Tobias E. Bjorck, . . . *Dissertatio gradualis, de plantatione ecclesiae svecanae in America, quam . . . in Regio Upsal. athanaeo* (Upsaliae, 1731). See: *The planting of the Swedish Church in America* . . . trans. and ed. by Ira O. Nothstein, Rock Island, Augustana College Lib., 1943.

[8] Although Franklin probably knew nothing of the Swedish dissertation, he had other contacts with Swedish theology and theologians. The famous Moravian Count von Zinzendorf appointed two of his clergymen, graduates of the University of Upsala, to carry on his mission among the Swedes in West Jersey. One of these, Olof Marlander, came from Sweden in 1737, and in 1743, while employed in Franklin's printing office, prepared Bechtel's catechism for publication in Swedish. It was published by Franklin in the same year. John W. Jordan, John Bechtel: His contributions to literature, and his descendants, *Penna. Mag. Hist. and Biog.* **19**: 146, 1895.

As to the cause of religion, I really think it will be best served by stopping the prosecution; for, if there be any evil tendency apprehended from the publication of that piece, the trial and punishment of the printer will certainly make it a thousand times more public, such is the curiosity of mankind in these cases. It is, besides, an old thing, has been printed before both in England and by Andrew Bradford here; but no public notice being taken of it, it died and was forgotten, as I believe it would now be, if treated with the same indifference.[9]

Perhaps Franklin's solicitude for Bradford led him to minimize the appeal of the piece. His belief that it would die and be forgotten was not borne out by subsequent events. Without making a specific search, I have found three other printings of the work. It appeared in the *Pennsylvania Chronicle,* May 22–29, 1769 (No. 124); the *Pennsylvania Gazette,* December 31, 1788 (No. 3057); and the *London Chronicle,* October 23–25, 1783 (No. 4201), and Franklin says that it had appeared previously in Pennsylvania and in London. There are minor verbal differences in the four versions which I have compared, but the substance is essentially the same. According to the version in the *Pennsylvania Gazette,*

In or about the year of our Lord, 1710, a Swedish missionary preached a sermon at an Indian treaty, held at Conestogoe, in which sermon he set forth original sin, and the necessity of a mediator; and endeavored by certain arguments to induce the Indians to embrace the christian religion: After he had ended his discourse, one of the Indian chiefs made a speech, in reply to the sermon; the discourses on both sides were made known by interpreters. The missionary, on his return to Sweden, published his sermon, and the Indian's answer. Having wrote them in Latin, he dedicated them to the university at Upsall, and desired them to furnish him with arguments to confute such strong reasonings of the Indian.

This account contains only a few minor inaccuracies. The sermon was delivered in 1699 or 1700, not in 1710 (or 1720 as some of the other newspapers have it), and the son of one of the missionary's friends, not the missionary himself, presented the Indian's discourse to the University of Upsala.

The Indian's reasonings in the deistical piece are substantially the same as those submitted to the faculty at Upsala. He asserted first that since his forefathers were under just as strong persuasion as were they themselves that a good life would be pleasing to God and entitle them to a happy state after death, both he and his ancestors must have received this revelation directly from heaven. Second, he argued that if their understandings are sufficiently illuminated to know that they must please God, it is unreasonable to believe that God will condemn them for not knowing the Christian Book of Revelation. Third, despite the minister's assertion that there are many precepts in the written revelation which they are entirely ignorant of, it is possible for God to manifest his will without a book. Fourth, since they are undeniably part of the creation of God, it follows that they are under his care. Fifth, even though it be admitted that God has forsaken them, he would not do so without a just cause. God may punish some, but he will never condemn the innocent with the guilty. Sixth, the Christians may know more than the Indians, but they are more depraved in their morals. Their doctrines must be judged by the badness of their lives.

The version in the *New York Gazette* omits the fifth and sixth points, but substitutes a summary of the story of the Garden of Eden and a denunciation of its doctrinal basis as blasphemous and impious. This is the version which Franklin follows in his *Remarks.*

The deists were undoubtedly shrewd in making use of this episode, but they could probably have made as much capital out of the Indians who pretended to be converted to Christianity as out of those who resisted it. Some Delaware Indians once artfully pretended to have been persuaded by the Calvinistic doctrines of Count von Zinzendorf. An indignant agent who crossed their path reported to the proprietors of Pennsylvania that on one occasion they wrote to the governor praying "that as they are become Christians and of the same religion with the white people they may be permitted to stay on their Lands and not to remove to live with Heathens," by which title they referred to the other members of their own nation.[10] These zealous converts, who subscribed themselves "Your Honour's brethren in the Lord Jesus," were catechised shortly after this petition and discovered to have very little knowledge of Christianity, if any at all.

Twentieth-century readers aware of the source of the deistical piece and of Franklin's *Remarks* realize that their portrayal of Indians has a strong basis in reality. Certain colonial readers, however, who had never seen the Swedish dissertation, regarded these literary Indians with sceptical scorn.

[9] May 14, 1752. Smyth, **3**: 88.

[10] Richard Peters to Thomas Penn, 1742, quoted by Boyd, *Indian treaties,* xxxiv.

The editor of the *New York Gazette* after being denounced for publishing the deistical piece, suggested, perhaps in self-defence, that the Indians therein are false, for real Indians are among the people who merely pretend to morality and are generally more prone to malice and vice than others. A defender of revelation in the *Pennsylvania Gazette* wrote that few would accept the piece as genuine because "it is so different from the Indian way of address."[11] Probably even Franklin himself as well as the deists who circulated the piece would have been surprised to know that the discourse was genuine and that Indians had actually used the arguments of natural religion against the doctrines of orthodox Christianity. Before declaring it genuine, we must of course have faith in the reliability of the Swedish minister's record and of Biörck's transcript, but since neither obviously would have any interest in inserting deistical arguments—in fact their natural tendency would be the opposite—there is no reason to suspect the authenticity of the account published at Upsala.

The version in the *Pennsylvania Gazette* is almost a direct translation, and that in the *New York Gazette* differs only, as has been said, in a treatment of the doctrine of original sin. Franklin's *Remarks* derives from the latter. He distilled the invective and heavy irony of the deistical piece into a light, good-humored parody, which appears opposed to orthodoxy only when viewed in relation to its source.

The Indian in the *New York Gazette* ridicules the grave countenance of the preacher and burlesques as follows the story out of his book:

> . . . When God created the first Man, he cast him in a deep Sleep, that he might take out one of his Ribs, to make him a Wife; (as if he had not sufficient of the same Materials, with which he made the Man of:) Man and Woman being made, he put them in a fine Garden; but the Woman fell in Conversation with a Serpent, who inticed her to transgress, by stealing some choice Fruit, that God had reserved for himself; and for this Offense, he drove them out of the Garden, and with-held all Means of Salvation from them, for the Space of Five Thousand Years: . . . this Man tells us . . . that we are contaminated, with the Theft of that first Woman, and involved in her Guilt; which has been the Subject of his Discourse this Day amongst us: *O horrible!* can the Man be drunk, or lost his Reason! or who made that Book, that contains that impious and blasphemous Stuff?

Franklin's style is more subtle, and his Indian is by definition more polite.

> "What you have told us," says he, "is all very good. It is indeed bad to eat apples. It is better to make them all into cyder. We are much obliged by your kindness in coming so far, to tell us those things which you have heard from your mothers. In return, I will tell you some of those we have heard from ours."

This is, of course, not a textual parallel. All that Franklin took from the deistical piece was the situation of an Indian philosopher disdainfully rejecting the discourse of the Swedish missionary.

For textual parallels we must turn to another source. In a work published by Franklin eighteen years before the *Remarks,* we find the verbal parent of the following legend which Franklin's Indian subsequently delivers.

> In the Beginning, our Fathers had only the Flesh of Animals to subsist on; and if their Hunting was unsuccessful, they were starving. Two of our young Hunters, having kill'd a Deer, made a Fire in the Woods to broil some Part of it. When they were about to satisfy their Hunger, they beheld a beautiful young Woman descend from the Clouds, and seat herself on that Hill, which you see yonder among the blue Mountains. They said to each other, it is a Spirit that has smelt our broiling Venison, and wishes to eat of it; let us offer some to her. They presented her with the Tongue; she was pleas'd with the Taste of it, and said, "Your kindness shall be rewarded; come to this Place after thirteen Moons, and you shall find something that will be of great Benefit in nourishing you and your Children to the latest Generations." They did so, and, to their Surprise, found Plants they had never seen before; but which, from that ancient time, have been constantly cultivated among us, to our great Advantage. Where her right Hand had touched the Ground, they found Maize; where her left hand had touch'd it, they found Kidney-Beans; and where her Backside had sat on it, they found Tobacco.

The substance of this cosmogony had previously appeared in a work by Franklin published in two issues of the *London Chronicle* under the heading, "Extract from an Account of the Captivity of William Henry in 1755, and of his Residence among the Senneka Indians six Years and seven Months till he made his Escape from them. Printed at Boston, 1766. 4to, Pages 160."[12] This work is longer than the *Remarks* and contains much more information about the Indians. The full story of creation it contains is more extensive than that in the *Remarks,* but the account

[11] No. 3060. January 21, 1789.

[12] Nos. 1798–99. June 23–25 and June 25–28, 1768.

of the beautiful young woman descending from the skies, here given the name of Manitta's daughter, is almost identical.

Nine *Oneida* Warriors passing near a certain hill, not far from the head of Sasquehanah, saw a most beautiful young Woman descend naked from the clouds, and seat herself on the ground upon that hill. Then they said, this is the great Manitta's Daughter, let us go to her, welcome her into our country, and present her some of our venison. They gave her a fawn's tongue broiled, which she eat, and thanking them, said, come to this place again after twelve moons, and you will find, where I now sit, some thing that you have never yet seen, and that will do you good. So saying she put her hands on the ground, arose, went up into the cloud, and left them. They came accordingly after twelve moons, and found growing where she had pressed the ground with her right hand, corn, where with her left hand, beans; and where her back parts had pressed it, there grew tobacco.

The only differences are superficial. In the *London Chronicle* account there are nine Oneida Indians instead of two hungry ones, the beautiful young woman is presumably more beautiful by being naked, she is identified as "the great Manitta's Daughter," and she makes the Indians wait only twelve moons instead of thirteen.

Three other parallels illustrate the close relationship between the *Remarks* and the *Captivity of William Henry*.

Captivity: The Indians having no letters, their women are the repositories of History; and are present for that purpose in all public councils, and at all treaties. It is their business to remember, and to transmit public facts and traditions, by relating them often to their children.

Remarks: The business of the Women is to take exact notice of what passes, imprint it in their memories, for they have no writing, and communicate it to their children. They are the Records of the Council, and they preserve tradition of the stipulations in Treaties a hundred years back.

Captivity: They think themselves the politest people in the world, as well as the wisest and bravest. . . . Contradiction, or a direct denial of the truth of what another says, is, among the Indians, deemed extremely rude.

Remarks: The politeness of these Savages in conversation, is, indeed carried to excess; since it does not permit them to contradict, or deny the truth of what is asserted in their presence.

Captivity: (The Indian to William Henry:) When young, it seems you were not well taught, you did not learn the civil behaviour of men.. . . You see I always believed your stories, why do you not believe mine?

Remarks: (The Indian to the Swedish minister:) My brother, it seems your Friends have not done you Justice in your Education; they have not well instructed you in the Rules of Civility. You saw that we, who understand and practice those Rules, believ'd all your stories; why do you refuse to believe ours?

Like much of the material in English periodicals at this time, William Henry's narrative purports to be an extract from a complete book, but no trace of such a book is to be found. Charles Evans' *American Bibliography* makes no mention of a William Henry or of a similar book on Indian captivity published in 1766. Furthermore, the Onondaga Indians, whose chieftain, Canassatego, explains primitive philosophy and ethics to William Henry, lived on consistently good terms with the English in Pennsylvania. Historical records give no hint of any armed conflicts or of English taken prisoner in 1755. Even though relations between the whites and the Iroquois in Pennsylvania grew strained in 1749, the two groups lived for the most part on amicable terms.[13] In April, 1756, Governor Robert Hunter Morris declared war against the Delaware Indians, but not against any of the nations mentioned in the *Captivity*.[14] Not until 1766 did any real war with the Six Nations take place. Franklin probably created the framework of an Englishman taken prisoner to gratify the preconceptions of his London readers. The lore of the ancient chief would have more allure if told beside a campfire to a white prisoner than if related at a trading post to a merchant or colonial commissioner. The *Captivity* is another hoax by Franklin similar to his *Dogood Papers* and his *Speech of Polly Baker*.

Evidence that the work is Franklin's is external as well as internal. Not content with publication in London, Franklin sent copies of the *Chronicle* to France with the object of having it printed there. His friend Dr. Jacques Barbeu Dubourg, whom he had met during his first visit to France, lived on close terms with the editors of *Ephémérides du citoyen, ou bibliothèque raisonée des sciences morales et politique* (1765–1772), a literary-economic journal devoted to promulgating principles of the physiocrats. Dubourg had already translated other works by Franklin for the *Ephémérides*, and it was probably he who prepared the narrative of William Henry for the French press. It appeared in the *Ephémérides* under the title

[13] *Indian treaties*, xxi, lx.

[14] *Ibid.*, lxxii.

"Mithologie et Morale Iroquoises." [15] The editors remarked that they had at first taken the account for a fairy story, but had continued their reading because it had been sent to them by Franklin. "Mais comme ce conte des Fées nous était adressé par le célebre BENJAMIN FRANKLIN, nous en avons achevé la Lecture." Here is convincing external evidence of Franklin's authorship. Incidentally, the present writer would never have noticed William Henry's relation, or attributed it to Franklin, had he not seen the *Ephémérides* in the Bibliothèque Nationale at Paris. One might examine the *London Chronicle* hundreds of times looking for Franklin items without pausing over the *William Henry* title.

To be sure, the editors of the *Ephémérides* do not specifically claim Franklin as author, merely that he sent them the work, but the evidence of Franklin's personal interest in its circulation combined with the internal evidence of its resemblance to the *Remarks* makes it almost certain that he was the author. Either Franklin wrote the *Captivity of William Henry* or he was guilty of copying from another author. Everything we know of Franklin points against the latter possibility. He was accustomed to printing his works in periodicals under fictitious names or initials, some of which were so well concealed that they were not identified until this century. He frequently copied whole sections of his own writings in new works (witness, for example, the sections from his letter to Collinson reprinted in the *Remarks*), but he did not copy from other authors for publication without making acknowledgment of his borrowing. To be sure, his bagatelle "The Ephemera" is based on a periodical essay published nearly sixty years previously, but in "The Ephemera" Franklin borrowed only a theme and not specific passages.[16, 17] The similarities between the *Captivity* and the *Remarks*, on the other hand, are too close to be the result of coincidence or unassisted memory. I shall henceforth assume, therefore, that there is no question about Franklin's authorship of the *Captivity of William Henry*.

The *Capitivity of William Henry* is more extensive than the *Remarks* and contains much more in theme and in substance than the amusing cosmogony quoted above. To foster the illusion that the work is actually an extract from a complete book of 160 pages, Franklin introduces his remarks with a supposed condensation of the unessential narrative details of the book.

> This Writer, who is an Englishman, gives a plain short account of his education in human learning at an academy in Northampton; his settlement in America as a trader with the *Ohio* Indians; his being surpriz'd and made prisoner at the breaking out of the late war; his spiritual change or conversion during his sickness and other afflictions; and then among a multitude of other particulars, relating to the Indians, says.

Immediately after this introduction begins the pretended direct quotation from William Henry's account. After three years of captivity, Henry succeeded in learning the Indian language and in so doing gained the respect of his captors. He frequently engaged in conversation with "Old Canussatego," a "Warrior, Counsellor, and the chief man of our village." This Canussatego (elsewhere in the work spelled Canasseteego) was in actual life the Onondago chief of the Indian treaty described at the outset of this article. Some of his speeches in the published treaties reveal a psychological shrewdness and genuine sly humor, not far removed from the traits of the philosophical seer of Franklin's work. In 1744 the real Canassatego was described in the "only pen picture available" as "a tall well-made man" with "a very full chest and brawny limbs. He had a manly countenance, mixed with a good-natured smile. He was about sixty years of age, very active, strong, and had a surprising liveliness in his speech." [18] The fact that the real Canassatego died in 1759 and that there is no record of a successor of the same name gives further proof, by the way, that the narrative of William Henry's captivity is pure fiction.

The real Canassatego was widely known for his skill in oratory, and Franklin in the *Captivity* amuses himself with the picturesque declamations of his fictional counterpart. Old Canassetego takes a great deal of pains to instruct William Henry in the principles of Indian eloquence,

> an art (it may seem strange to say it, but it is strictly true) carried much higher among these Savages than it is now in any part of Europe, as it is their only po-

[15] 1769, **6** (2): 56–78. Citation of the Ephémérides is complicated because the work is not bound uniformly. I am citing the set in the Bibliothèque Nationale at Paris.

[16] See Verner W. Crane, Certain writings of Benjamin Franklin on the British Empire and the American Colonies, *Pap. Bibliog. Soc. Amer.* **28** (1): 1–27, 1934.

[17] Smyth, **7**: 206–209.

[18] W. H. Egle, ed., *Journal of the treaty at Lancaster in 1744 . . . by Witham Marshe*, 12, Lancaster, 1884. Cited in *Indian Treaties*, xxxvii. In this work the name of the Indian chief is spelled "Canasetgo"; in Franklin's *Remarks concerning the Savages*, "Canassatego"; and in *Captivity of William Henry*, "Canussatego" and "Canasseteego."

lite art, as they practice it from their infancy, as every thing of consequence is transacted in councils, and all the force of their government consists in persuasion.

Franklin acquired his first-hand knowledge of Indian rhetoric and ritual by printing thirteen Indian treaties as well as by serving himself as commissioner to negotiate with Indians. Treaties followed an elaborate ceremony, prescribed by the Indians. In ritual, if in nothing else, the Indians imposed upon their white conquerors. The representatives of the Pennsylvania government would open a council with a speech of several articles and present each of the Indians a string of wampum. Then one of them, speaking for them all, would repeat the articles "from a memory as accurate as written minutes" and reply to them, distributing wampum in his turn. The councils "were grave and punctilious, as orderly as a trial before a high court of law, as straightforward in action as a good play." [19]

Perhaps the plain and prosaic side of Franklin became a little tired with this formalism. He seems at least to take special pains to ridicule Indian eloquence in the following description of Old Canassatego applying his principles of rhetoric:

> Then raising his voice, and entering into the council stile and manner of speaking, and with that modulation which I may call the *quoting tone*, being what they use when *repeating* messages, treaties, or any thing that has been said by others in former times, distant places, or preceding councils; a tone so particular, that if you come into a council in the middle of a speech, you can tell whether the person speaking is delivering his own sentiments, or reciting those of another; this tone having the same effect in their speeches, and answering nearly the same end, with our marginal inverted comma's in writing, to distinguish borrow'd passages quoted as authorities; only that the Indians have three differences in the quoting tone, none of which we have in writing, viz., the *approving* accent, the *disapproving*, and the *uncertain* or *doubting;* and that there is something measured or musical in all these tones. I say, *Canasseteego*, in the quoting or historical tone, with the *approving* accent, and with an air of great authority and dignity, went on with his account of the manner in which his country was made and peopled.

All that Franklin retained in the *Remarks* of this discourse on oratory is the statement, "All their Government is by Counsel of the Sages. . . . Hence they generally study Oratory, the best Speaker having the most Influence."

How much of Franklin's attitude toward Indian oratory is admiration, how much amusement and how much absolute ridicule is impossible to say. Probably Franklin alternately experienced all three moods as he participated in treaty-making. European readers of his *Captivity*, however, no doubt accepted his comments as commendatory.

When not orating, Old Canassatego occupies a role similar to that of the King of the Brobdignagians in *Gulliver's Travels*. The Indian chieftain would often enquire of his captive concerning "our wars, history, customs, arts, &c. and sometimes about our religious opinions." As Gulliver wished for the tongue of Demosthenes or Cicero that he might justly celebrate the praise of his own dear native country, William Henry regrets his incapacity to reply to the chief's questions because in his youth he "had so unhappily refused the advantage . . . of acquiring store of divine knowledge under the pious instructions of Dr. Doddridge."

One day while William Henry contemplated the Gulliverian task of making the Indians perceive the wisdom and goodness of European "regulation of commerce, by which one nation proposes to make advantage to itself in distressing the trade of others," a young warrior begins a discourse on creation, the one which is printed above concerning the beautiful young woman descending from the clouds. In the *Captivity* this cosmogony is not allowed to go unchallenged, as it is in the *Remarks*, for all the young Indians of the *Captivity* laugh heartily at the origin of tobacco described therein, and Old Canassatego rebukes the narrator for telling this foolish Oneida tale to their white captive. "If you tell him such tales, what can you expect but to make him laugh at our Indian stories as much as you sometimes do at his." To atone for the foolishness of the young warrior, Old Canassatego tells the true story of the beginnings of the country. Before doing so, however, he corrects William Henry's notion that there is but one "great good Manitta," usually called the "Great Manitou" in other Indian literature.

> If there were but one, how unhappy must he be without friends, without companions, and without that equality in conversation, by which pleasure is mutually given and received! I tell you there are more than a hundred ([Franklin's note:] They commonly use a hundred to express any great unknown or indeterminate number.) of them; they live in the sun and in the moon; they love one another as brethren; they visit and converse with each other; and they sometimes visit, though they do not often converse with us.

[19] *Indian Treaties*, xiii.

> Every country has its great good Manitta, who first peopled that country.

There is nothing of this plurality of gods repeated in the *Remarks*, but it is nevertheless a doctrine familiar to Franklin. In his *Articles of Belief and Acts of Religion*, written in 1728, Franklin expressed the belief in the existence of "many beings or Gods, vastly superior to Man." In his youth Franklin conceived merely of one god for each sun and its system of planets instead of one god for each country, but the notion of plural deities is the same. The *Captivity of William Henry*, aside from its intrinsic literary merits, has the biographical significance of showing that at the age of sixty-two Franklin had retained his interest in the doctrine of a plurality of worlds and of gods. To be sure, he may be merely reporting the facts of Indian religion, but he does so with interest and apparent sympathy.

After establishing a "good Manitta" to people every land, Old Canassatego continues with an account of the peopling of Akanishionegy, the land of the five Indian nations, an account which combines primitive animism with a crude form of scientific evolution. Having dedicated the land to red men, who are the best of men, the good Manitta strewed the fertile fields of Onondaga with five handfuls of red seeds, like the eggs of flies.

> Little worms came out of the seeds, and penetrated the earth, where the spirits who had never yet seen the light entred [*sic*] into and united with them. Manitta watered the earth with his rain, the sun warmed it, the worms with the spirits in them grew, putting forth little arms and legs, and moved the light earth that covered them. After nine moons they came forth perfect boys and girls. Manitta covered them with his mantle of warm purple cloud, and nourished them with milk from his finger ends. Nine summers did he nurse, and nine summers more did he instruct them how to live.

One does not have to be a Freudian to wonder how much of this imagery Franklin intended as sexual and embryological. On the surface, however, it remains simply a myth of cosmogony—the Five Nations springing from the five different handfuls of seeds. In a loose paraphrase of the second chapter of Genesis, Old Canassatego pictures the great good Manitta assembling his five children, naming them (Mohocks, Oneidas, Sennekers, Cayugas, and Onondagoes), and providing each with a characteristic foodstuff. Manitta crowns his act of creation with a forecast of things to be.

> The bodies I have given you will in time grow old and wear out, so that you will be weary of them; or from various accidents they may become unfit for your habitation, and you will leave them. . . . I have enabled you therefore, among your selves, to produce new bodies, to supply the place of old ones, that every one of you when he parts with his old habitation may in due time find a new one, and never wander longer than he chuses under the earth, deprived of the light of the sun.

In a note to this passage, Franklin writes:

> They believe spirits ramble about under the earth, in a country where there is only a kind of twilight. That in that country are also the spirits of birds, beasts and fishes, and even of trees and plants. That all these spirits, a spirit can see and handle, without eyes or hands; but if he comes again above ground, he finds he cannot see the sun, or move even a grain of sand without eyes and hands; and therefore, he seizes the first opportunity of getting a new body, by entering and possessing an embrio just forming in its mother's womb; from which moment he forgets every thing but love to his country. The returning spirits of birds, beasts and fishes, they say do not forget every thing: The birds retain the memory of the manner of walking, flying, copulating, and building of nests; the beasts of walking, coupling, swimming, &c. and the fish of swimming, and other actions which the great spirit first taught them, and therefore now need no fresh teaching in those particulars.

We may well pause at this point to inquire how much of this system of metempsychosis and preexistence Franklin intended seriously and how much as straight burlesque. We shall never arrive at a completely satisfactory answer to this query, but the evidence of Franklin's early *Articles of Belief* shows that at one time in his life he sincerely accepted doctrines very much like those of Old Canassatego. His account of spirits without bodies rambling under the earth is, moreover, based on actual Indian lore. In one of his printed Indian treatises, a chief speaks of "all those then under the Ground, who had not yet obtained Eyes or Faces," and a parenthetical explanation is offered that this refers to "those unborn." [20]

Franklin's attitude toward Indian religion is certainly no less sympathetic than his attitude toward orthodox Christianity which he treats in the vein of the deistical essay in the *New York Gazette*. William Henry refuses to let pass Old Canassatego's account of creation without remarking that it is subject to great uncertainty, being trusted to memory only, from woman to woman

[20] *A treaty held with the Ohio Indians, at Carlisle, in October, 1753*, 6, Phila. 1753. In *Indian treaties*, p. 128.

through many directions; whereas the Christian account "was written down by direction of the great spirit himself, and preserved carefully in a book, which was never altered, but had ever remained the same, and was undoubtedly the truth." There seems to be more of irony than sincerity in this panegyric. Old Canassatego's answer, which Franklin later repeated in the *Remarks,* is a humorous tribute to Indian politeness. Old Canassatego laments that William Henry has failed to learn the civil behavior of men. Despite his long residence among the polite Indians, he is almost as rude as when he first arrived. "You see, I always believed your stories, why do you not believe mine?" Some of the younger Indians make excuses for William Henry, saying that he will be wiser in time. They conclude with an observation illustrating their politeness and respect, a remark as amusing as anything in the whole *Captivity,* but one which Franklin did not repeat in the *Remarks.* They say that William Henry's stories "indeed might be best for white people, but Indian stories were undoubtedly best for Indians."

The *Captivity* contains no more satire of the fundamental doctrines of orthodox Christianity than does the *Remarks,* and in both works the reflections on Christianity are subtler than those in Franklin's model—the deistical piece in the *New York Gazette.* His cosmogony of the naked woman from the clouds, however, is certainly not a serious substitute for the first and second chapters of Genesis. Neither is it a masterpiece of primitive wisdom, as it was interpreted by the French editors. Franklin is having as much fun with the deists as with the orthodox. The whole work is supremely eclectic in its humor. But although Franklin has rejected orthodoxy, he is still speculating on the plurality of worlds. Realizing the ethereal nature of his speculation, he finds amusement in his own attempts to solve the problem of life and individual identity. Metempsychosis and preexistence are no more seriously intended than is the character of Old Canassatego himself. The latter is so wise and so philosophically erudite and he expresses himself with such vigor and grace that he could almost serve as an Indian Franklin. Yet just as Franklin gave some realistic traits and practical comments to the ancient chief, he probably thought seriously about some of the esoteric metaphysical concepts he introduces. He may, on the other hand, have been merely lightly mocking himself for retaining such esoteric notions, the persistence of which may have both annoyed and amused him.

In the remainder of the work, Franklin discards metaphysics and tackles the economic system. Here his distress at moral deficiencies is genuine and earnest. He introduces an apologue concerning the discord introduced by economic competition and greed which in its sombre invective resembles the style of Swift. The five nations lived happily as long as they communicated "freely to each other as their wants required, all the good things that had been given them." This formula upon which the good Manitta established order among the five nations seems to be socialistic, but Franklin probably intended his apologue to inculcate nothing more drastic than the virtues of free trade—a principle of the physiocrats. The broader overtones of the parable, however, are inescapable. The great evil Manitta came among the five nations and caused them to say,

> We abound in corn which our brothers have not; let us oblige them to give us a *great deal* of fruits, beans, roots, squashes, and tobacco, for a *very little* corn; so shall we live in idleness and plenty, while they labour and live hardly. And in the same manner spoke the other nations. Hence arose discord, animosity, and hatred; insomuch that they were on the point of lifting the hatchet against each other, and miring the earth with brother's blood.

Great anger seized their father in the sun when he saw this evil. He covered the land with a thick blue and red cloud and hurled denunciation in thunder.

> Wretches, said he, did I not freely give to each of you different kinds of good things, and those in plenty, that each might have something in his power to contribute to his brother's happiness, and so increase the happiness and strengthen the union of the whole; and do you now abuse those gifts to oppress each other; and would one brother, to make himself *in imagination* more happy, make four brethren *in reality* more miserable.

The great Manitta added drastic deeds to his words. The whole order of nature was reversed. The sun gave forth darkness instead of light; the rivers ran backwards, carrying all their fish to the mountains and leaving their beds dry; the rain ceased; plants perished and animals deserted the land; the afflicted people, forced by famine to leave their native land, wandered among neighboring nations, begging their food "from those who despised them for their late wickedness to one another." After the passing of nine summers, however, they finally rejected the counsels of the evil spirit. "They began mutually to feel and to pity

one anothers misfortunes; they began to love and to help each other." Seeing their amendment, the nations among whom they lived offered to adopt them, but they preferred to retain their national identity, hoping that the great Manitta would someday restore their land. Their hopes were justified. The great Manitta, rejoicing in their change of heart, restored the light, moisture and fertility to the land. The five nations returned to their ancient seats. "From that time down to the present day, it has been an inviolable rule and custom among the nations, that *every Brother is welcome to what a Brother can spare of the good things which the great Spirit has caused to spring for him out of the earth.*" This is the moral conclusion of Canassatego's tale, but its economic significance is somewhat enervated by the qualifying phrase "what a Brother can spare." The wealthiest nation or individual can always deny his poorer brother on the grounds that he has nothing to spare. Franklin makes a particular application, however, to contemporary economic problems. Speaking again through Old Canassatego, he delivers a specific plea for free trade. The English and French, "though they called the Indians brothers, had long practised the same wickedness toward them, making every thing dear that they exchanged with them, and even the things they [the English and French] exchanged with one another." The wickedness toward the Indians refers to the greed of the whites and to their sharp and unscrupulous practises in trading with the Indians. The reference to the English and French making things dearer for each other refers to the duties laid on each other's commodities. New York makes Canada pay dearer for strouds and blankets, and Canada makes New York pay as much dearer for beaver.[21] "What, at best, can either of them get by this, but his own inconvenience and the other's ill-will?" But this is not the worst of it. "The great spirit of the white people" is angry with the French and English just as the great Manitta had been angry with the Five Nations, and they are now engaged in a war, brother against brother, in which they "destroy their own habitations, and bring misery on both their countries."

In this section Franklin is repeating the same principles of free trade he had expressed in the *London Chronicle* two years previously in his well-known letter "On the Price of Corn."[22] In suggesting that free trade could have prevented the commercial wars between the English and the French, however, Franklin uses much stronger language than in the mild and good humored letter "On the Price of Corn."

The concluding paragraph of the *Captivity* returns to the method of *Gulliver's Travels*. With obvious irony, William Henry disclaims sympathy with the Indians and categorically asserts the superiority of western orthodox beliefs.

> Now it is well known that some who have before me been among these Indians, have reported highly of their stories, as if there were something super-excellent in them. I have therefore given this story of theirs at full length translated as well as I am able; and I can faithfully assure my readers it is one of their very best, by which may be seen the miserable darkness these poor creatures labour under, and how far inferior their best instructions do appear when compared with the unerring oracles that we possess, and the histories contained in them.

For the first time in the work, Franklin seems to have lost his tolerant good-humor. He is almost savage in his denunciation of English smugness and stupidity. This paragraph in particular invites comparison with another work of Franklin's concerning the Conestogoe Indians. This is his *Narrative of the Late Massacres in Lancaster County, 1764*, concerning an infamous attack by murderous whites on a peaceful settlement of twenty inoffensive Indians.[23] On their first raid fifty-seven armed whites slaughtered six defenceless natives in their huts, the only Indians then in the village. The magistrates of Lancaster then brought the remaining Indians into town and placed them in the Workhouse for their protection. The same men, hearing of this, again assembled, broke down the door, and murdered the remaining Indians as they knelt before them in family groups, protesting their innocence. Franklin's narrative of these events is an impassioned outcry against the base inhumanity of his fellow English.

> O, ye unhappy Perpetrators of this horrid Wickedness! reflect a Moment on the Mischief ye have done, the Disgrace ye have brought on your Country, on your Religion, and your Bible, on your Families and Children! . . . You have imbrued your Hands in innocent Blood; how will you make them clean? The dying Shrieks and Groans of the Murdered, will often sound in your Ears: Their Spectres will sometimes attend you, and affright even your innocent Children!

[21] Although English readers were probably not familiar with the term, a stroud was a blanket specially manufactured for trade with the Indians.

[22] Smyth, **5**: 534–539.

[23] Smyth, **4**: 289–314.

Such rhetorical and emotional intensity is rare in Franklin's work. Stricken with the brutality of the mass crime enacted only a few days previously, he appeals passionately to the citizens of Pennsylvania to treat the Indians like human beings and accord them common justice.[24]

The *Captivity of William Henry,* probably written two or three years later, has in part the same purpose, but Franklin's emotions have had time to cool. He argues for fair play for the Indian, but he does so dispassionately and with a comic touch. Only in the final paragraph does he regain the intensity of the earlier work. His purpose, moreover, is manifold. As we have already seen, he is concerned with religious and economic concepts as well as with the welfare of the Indian. He is seeking to expose both the narrowness of orthodox Christianity and the pretentiousness of deism. Both are subjected to good-humored ridicule. In exposing the evils of commercial rivalry, he condemns individual greed and selfishness in addition to advocating the economic principle of free exchange.

In these philosophic and economic overtones we find the essential difference between the *Captivity* and the *Remarks.* The *Captivity* is a satire on universal human frailties exemplified by the religious myths of braves and missionaries and the economic policies of the British Empire. The *Remarks,* on the other hand, is an amusing local color sketch of Indian character and customs. The *Captivity* bristles in the satirical vein of Jonathan Swift deriding the Lilliputians. The *Remarks* glows in the tolerant spirit of Joseph Addison discoursing indulgently on fans and patches.

The cosmogony of the beautiful woman descending from the skies is of course common to both works, but serves two entirely distinct purposes. In the *Captivity* it is derided, called a foolish Oneida tale, and contrasted with the rhapsodical discourse on metempsychosis and the origin of the five nations. In the *Remarks* it stands by itself with no comment of any kind. Perhaps in the interim between the publication of the two works, Franklin realized that regardless of its absurdity or disharmony with his own religious beliefs, the tale has great narrative and character interest. In the *Remarks* it is told entirely for its own sake as an amusing anecdote parallel to the preliminary narrative of the Indians refusing the academic advantages of the college at Williamsburg.

Both the *Captivity* and the *Remarks* criticize the whites for their sharp trading practices with the Indians, but again Franklin has two separate purposes. In the *Captivity* he reflects on the whole European commercial structure and suggests the superior advantages of the physiocratic system over the mercantilist. In the *Remarks* he merely tells another tale, less to denounce the English commercial system than to illustrate the droll appeal of primitive Indian wisdom. Canassatego is again the principal actor. Observing that the whites in their settlement refuse to trade with him prior to attending their religious service and that after their pious duties have been performed they all have agreed on a uniform low price for his beaver skins, he concludes "that whatever they pretended of meeting to learn *good Things,* the real purpose was to consult how to cheat Indians in the Price of Beaver." Franklin is still reflecting on sharp trading practices, but his primary purpose is not to attack evil, but to tell another humorous story. His readers in London, where the *Remarks* was first published commercially, were probably less disturbed by the duplicity of the English traders than charmed by the primitive reasoning of Canassatego.[25]

Both the *Captivity* and the *Remarks* have elements of realism based, as we have seen, on Franklin's first-hand knowledge of Indian life. In a sense there are more realistic details in the *Captivity* since Franklin is under the literary necessity of imparting verisimilitude to his work in order to enforce the satirical elements in his narrative.

[24] In a note affixed to the *Remarks,* Franklin gives a number of examples of "barbarian" tribes noted for the virtue of hospitality parallel to a more extended series of examples of the same thing in *Narrative of the Late Massacres.* Smyth, **4**: 299–306.

[25] The *Remarks* was first published at Franklin's press in Passy in separate French and English versions. Especially interesting is the emphasis on politeness in the French title: *Remarques sur la politesse des sauvages de l'Amérique septentrionale* (L. S. Livingston, *Franklin and His Press at Passy,* 49, N. Y. Grolier Club, 1914). The *Remarks* was later printed in several editions in the same volume with *Information to Those Who Would Remove to America.* Presumably Franklin did not intend either piece for general publication in 1784. After learning of their appearance, he apparently wrote to Benjamin Vaughan to inquire concerning the circumstances of their being printed. The latter wrote to Franklin on November 21, 1784: "I know not *who* published your pieces on the Indians & on Imigrations, nor have I yet seen them. The latter piece the Abbé Morellet sent Lord Shelburne, from whom I had it; The Bishop of St. Asaphs' family afterwards had my whole packet of your pieces for many weeks. This is the only information I can now give you on this head." This letter is to be found in the Bache Collection of the library of the American Philosophical Society.

But these passages are realistic only in the sense that certain sections in *Gulliver's Travels* specifically introduced for verisimilitude are realistic. The *Remarks,* in which both the targets and the tone of satire are greatly reduced and the emphasis is shifted to narrative and character delineation, is the work which when viewed as a whole creates the impression of realism. The *Remarks* is superior also in the quality of unity. In the *Captivity* Franklin not only has a divided purpose of satire and narrative, but vacillates between ridiculing Indians and attacking whites. In the *Remarks* Franklin tells with a uniform tone of good humor three amusing anecdotes of Indian character, cleverly integrated and skillfully directed toward a universal audience. Whether the reader has any interest at all in Indians as a race, whether he is a native of Europe or America, of the eighteenth century or the twentieth, he will find amusement in the mythology, the quaint speech and simple wisdom, and the polite responses of Franklin's Indians.

Franklin's contemporary English readers may have taken the narrative as absolute realism, and some later readers have taken it as unadulterated satire. We now see from Franklin's sources that Franklin's Indians have a firm basis in reality, but we also see from Franklin's manipulation of his sources that they have been subjected to the power of transformation in Franklin's imagination.